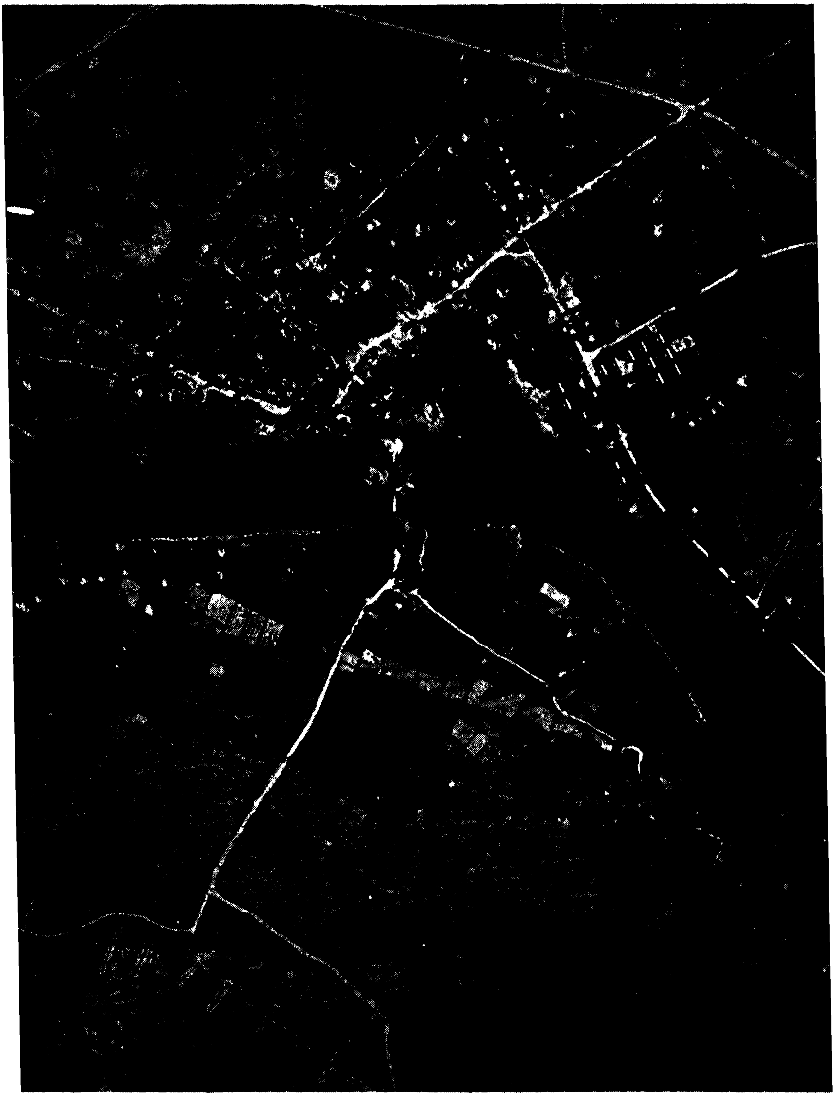
NEW WEAPONS FOR AIR WARFARE



Official Photo U. S. Army Air Forces

Tenth Air Force demolishes Taungup Pass Bridge in Burma with Azon bombs

SCIENCE IN WORLD WAR II

Office of Scientific Research and Development

New Weapons for Air Warfare

Fire-Control Equipment, Proximity Fuzes, and Guided Missiles

VOLUME EDITOR

Joseph C. Boyce

SPECIAL ASSISTANT TO THE CHAIRMAN
NATIONAL DEFENSE RESEARCH COMMITTEE
PROFESSOR OF PHYSICS, NEW YORK UNIVERSITY

WITH A FOREWORD BY

Richard C. Tolman

VICE-CHAIRMAN, NDRC
PROFESSOR OF PHYSICAL CHEMISTRY AND MATHEMATICAL PHYSICS
CALIFORNIA INSTITUTE OF TECHNOLOGY

With Illustrations

An Atlantic Monthly Press Book

Little, Brown and Company • Boston

1947

FIRST EDITION

Published September 1947

ATLANTIC-LITTLE, BROWN BOOKS
ARE PUBLISHED BY
LITTLE, BROWN AND COMPANY
IN ASSOCIATION WITH
THE ATLANTIC MONTHLY PRESS

Published simultaneously
in Canada by McClelland and Stewart Limited

PRINTED IN THE UNITED STATES OF AMERICA

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This history of OSRD is intended to be a survey of the way in which certain weapons were developed or improved, and is not intended to be a complete documentary record of the making of the inventions that happen to be mentioned in it. Therefore the dates used, which serve to outline the general chronology, do not necessarily establish dates of conception of inventions, of their reduction to practice, or of occasions of first use. Moreover, in the interest of simplicity, credit for the development of a device is frequently given to a group identified by the name of its leader, rather than to the individual who was the actual inventor.

The authors and editor of this volume do not receive any royalty from its sale.

The material in this volume was prepared by the authors under the direction of the Chiefs of the following administrative divisions:

Office of Scientific Research and Development

SECTION T

L. R. Hafstad, Vice-Chairman
J. A. Hynek, Author

National Defense Research Committee

DIVISION 4

A. Ellett, Chief
J. S. Rinehart, Author

DIVISION 5

H. H. Spencer, Chief
E. W. Phelan, Author

DIVISION 7

H. L. Hazen, Chief
S. H. Caldwell
S. W. Fernberger
I. A. Getting
L. M. McKenzie
E. J. Poitras
G. R. Stibitz, Authors

FOREWORD

THIS VOLUME of the series on the history of the Office of Scientific Research and Development describes the work of those sections of that office and of its subdivision, the National Defense Research Committee, which were mainly concerned with the development of new devices for controlling the behavior of projectiles and other missiles. The devices developed included systems of fire control for the correct aiming of guns on the target, proximity fuzes for use on shells, bombs, and rockets to secure explosion at the right distance from the target, and "guided missiles" which, either by manual or by self-directed control, can actually be made to seek out the desired target. It will hence be appreciated that the volume deals with some of the most elaborate, novel, and important developments in the modern art of warfare. Since many of the devices were especially important in the field of air warfare, the volume has been given the descriptive title "New Weapons for Air Warfare."

The volume was written by the authors listed on page viii, under the direction of the administrative Chiefs there named. It was edited by Joseph C. Boyce, Special Assistant to the Chairman, NDRC. Particular appreciation must be expressed for the work of the Editor, who has written some of the chapters *ab initio,* has improved or rewritten others, and has tied the whole together into a coherent presentation.

As I read the volume over — even in partially completed manuscript form — it brings pleasure to find that the men concerned in the writing have performed their task both adequately and interestingly. We get not only a competent account of the course of many diverse developments, but we get a sense of the devotion of the scientists and engineers who — to the point of exhaustion — put their best into the work to be done for their country, and we find pungent phrases that recall once more the successive days of fear and confusion, of organization and progress, and finally of achievement and victory.

After two introductory chapters, the first part of the volume describes the development of fire-control devices and systems which was undertaken at first by a section of Division D, NDRC, and then continued

after the reorganization of NDRC by the five — later six — sections of Division 7, to a considerable extent with the help of the same scientists and engineers. The accomplishments of these men reflect great credit on themselves, and played a great role in the successful outcome of the war. The detailed story of their many diverse activities, too many even to be listed here, will be found in the text. Mention may be made, however, of an important outcome of one of these activities. This was the development, through contract with the Bell Telephone Laboratories, of the T-10 electric director which was then standardized by the Army as the famed M-9. Successfully used throughout a great part of the war, it was this director in the last year of fighting — employed in conjunction with two other OSRD developments, the SCR-584 radar for the location of targets and the VT proximity fuzes for the explosion of shell at the target — which solved the great menace of buzz-bomb attacks against London by shooting the bombs down as they came in over the coast.

The second part of the volume describes the development of proximity or influence fuzes for detonating the explosive heads, carried by missiles, just at the right instant when they come under the influence of the target. From a practical point of view, two types of fuzes may be distinguished according as they are carried on rotating missiles such as shell, or on nonrotating missiles such as bombs and rockets. In the first case the electrical currents for operating the components of the fuze can be supplied from a so-called "reserve battery," which does not come into action until the time of firing, and then makes use of the centrifugal force of rotation to distribute the electrolyte properly to the battery. In the second case, where centrifugal force is not available, use must be made of miniature dry cells which have a severely limited shelf life, or use must be made of appropriately designed, miniature, air-driven dynamos.

The development of proximity fuzes for use on rotating shell was carried out by Section T, at first a part of Division A, NDRC, and later a separate section of OSRD. This development was one of the outstanding contributions to military art made in the war. It was no mean achievement to devise a complete radio sending and receiving set that could be fitted into the nose of a shell and withstand the shock of firing. The original impetus for work on these fuzes came from the Navy,

and much of the expense for their development was carried by funds transferred to OSRD from the Navy's Bureau of Ordnance. The earliest employment of these fuzes was for the protection of warships against aerial attack, an extremely important use. Later, by the time of the Battle of the Bulge, when secrecy restrictions against use over land had been lifted, they were used against enemy ground forces with a devastating effect because of the consequent explosion at an appropriate distance above ground. Their use, in conjunction with other OSRD developments, in defeating buzz-bomb attacks has already been mentioned.

The development of proximity fuzes for use on nonrotating projectiles was the responsibility of Section E of Division A, which after the reorganization became Division 4 of NDRC. The first development, successfully completed through the stage of manufacture by Section E, was carried out, at the request of the Army, for fuzes to use on their 4.5-inch rockets. Owing, however, to delays in the Army rocket program, these fuzes were never taken off the shelf. The next development of fuzes by the Division was for use on bombs, to secure detonation above ground at the height of greatest effectiveness. Air-driven dynamos were successfully engineered for supplying the electrical power to these fuzes, a noteworthy achievement in view of the necessary limitations on space available and on the internal generation of "noise." Bombs provided with such fuzes were an effective weapon for dropping from airplanes against enemy personnel and appropriate installations. The first combat use of this weapon was against Iwo Jima in February 1945. This was followed by extensive use in the Pacific area during the remaining months of the war. They were also given combat use in the spring of 1945, by the Ninth Bomber Command against Western Europe, by the Fifteenth against Southern Europe. At the end of the war, Division 4 was engaged in the development of proximity fuzes for use on nonrotating mortar shells, which were planned for employment in enormous numbers in the contemplated invasion of the Japanese home islands.

The third and last part of the volume describes the development of guided missiles. The NDRC work on such weapons was concerned with the guiding of bombs dropped from airplanes. One line of work, initiated in Section E, Division A, led to the development of highly maneuverable gliders, carried under the wings of the mother plane,

each itself carrying a bomb inside. Another line of work, initiated in Section D-3, Division D, led to the development of bombs, of dimensions near enough to the conventional to be carried in ordinary bomb racks, and provided with small control surfaces giving sufficient maneuverability to change a near-miss into a hit. The development of both types was carried to completion by Division 5 after the reorganization of NDRC.

Two kinds of radar-controlled glide bomb were developed by NDRC and manufactured under combined NDRC and Navy auspices. The first of these, Pelican, was provided with a radar receiving set giving the information needed to guide the weapon to a target illuminated by radar from the mother plane. The second, Bat, was provided both with a radar receiving and with a radar sending set, thus being able itself to illuminate the target. Of these, Pelican appears to have been the more satisfactory weapon; it was simpler; it was the first of the two to be developed and manufactured; it had the longer range up to twenty miles from the target; and on approach to the target it had only to contend with a changing signal strength increasing with the inverse second rather than with the inverse fourth power of the distance, as in the case of Bat. The difficulty of having to illuminate the desired target from the mother plane was only a minor one for a kind of operation that would in any case need specially trained squadrons. The Navy, however, preferred to wait for Bat, and this weapon toward the end of the war saw a small amount of combat use against shipping in the Pacific, with some interesting and successful results.

Three kinds of bombs were developed and produced by NDRC which could be dropped from planes in a conventional manner, using a bombsight for aiming, and then using small control surfaces to change near-misses into hits. The first of these to reach completed development, Azon, could be controlled in azimuth only. It was provided with a powerful tail flare so that it could be seen by the bombardier and then directed with the help of a radio link so as to secure hits on long, narrow targets such as bridges. A second similar weapon, Razon, which could be controlled in range as well as azimuth, reached complete development later. Finally, a target-seeking bomb, Felix, was also developed which would of itself home on the desired object. Azon was given appreciable combat use in Italy, and also in Burma, where it

made important contributions to the liberation of that territory.

I cannot bring this foreword to a close without mentioning the names of certain Army and Navy officers whose characters will always stick in NDRC memories as those of wise and good men who did all they could to help: Major General J. A. Green and Colonel W. S. Bowen, who actually believed in the improvement of fire control; Captain G. C. Hoover, the late Captain S. A. Shumaker, and Admiral W. S. Parsons of the Navy and Colonel H. S. Morton of the Army, without whom there might have been no proximity fuzes; and Brigadier General H. M. McClelland and Captain D. P. Tucker, U.S.N., who really wanted guided missiles to work.

The reader of this volume will find therein many lessons instructive for the conduct of war and for the conduct of peaceful affairs as well. It is our sincere hope that these lessons will find use in times of peace, but that the affairs of men will be so ordered, and the spirit of men so directed, that they will not be needed for the prosecution of another war.

RICHARD C. TOLMAN
Vice-Chairman, NDRC

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CHAPTER I

INTRODUCTION

THE RISING TEMPO in air warfare brought into combat a considerable number of new weapons including proximity fuzes, guided missiles, and new devices in fire control. In the early days of the war, emphasis was upon defensive devices, particularly for use against enemy bombers. The power of the Luftwaffe had ravaged Warsaw and Rotterdam and was turned back at London only after a great toll had been paid. The air attack by Japan at Pearl Harbor, followed so quickly by the sinking of the *Repulse* and the *Prince of Wales,* proved that fleets as well as cities were vulnerable. American antiaircraft defenses were initially meager in quantity and inferior in quality. Great effort was expended toward removing this deficiency.

As air power grew in Britain and America air combat increased in importance, and finally attack from the air was carried into enemy territory. These successive phases of war called for new types of weapons which had to be developed and produced in time for each type of operation. Offensive weapons came into use but defensive weapons were still needed in many operations. Beachheads required special protection at each invasion. Certain lines of communication, such as the route to Murmansk, were long subject to air attack. Special antiaircraft devices that had served at Anzio and with the fleet were joined together as a powerful defense against the buzz-bomb at London and later at Antwerp.

The responsibility for the development of new weapons rested initially solely with the Army and Navy. Commencing in June 1940 that responsibility was shared with a civilian agency of the Government, the National Defense Research Committee. A year later NDRC became part of the larger Office of Scientific Research and Development. NDRC and OSRD projects covered a wide range of new weapons for air warfare as well as those for use on the ground and at sea. This volume will confine itself to those projects on fire control, proximity fuzes, and guided missiles. In two of these subjects it will not even cover the

full range of American developments since considerable work on fire control and guided missiles was done by the Army and Navy apart from the NDRC-OSRD activities, in which of course there was close Service liaison and co-operation. The story of other developments by NDRC is told in other volumes of this series and some of them, such as radar and rockets, played vital roles in air warfare. Not all of the devices described in this volume were used for air warfare — some fire-control equipment was designed for other applications and the proximity fuze found important use in ground combat. Naturally, not all of the development projects had been completed or brought into combat use when the Japanese war came to such a spectacular conclusion. In the story of research and development there is often as much to be learned from unsuccessful efforts and incomplete projects as from the more satisfying achievements of new devices proved in combat.

The fields of activity to be covered in this volume may profitably be summarized at this point. Fire-control devices comprise the various mechanisms used to make shooting more accurate. They include a wide variety of equipments to determine the present position (and velocity) of the target and to compute the proper direction to point the gun to hit the target. Fire control also includes the proper aiming of bombs, rockets, torpedoes, and depth charges. Range finders to measure target position may be optical instruments or may depend on radar. If the target is moving this computation must predict its future position for the shell to reach it. They also include devices known as servomechanisms which automatically point the gun in accordance with these computed instructions. The computing devices (called directors in Army parlance) are quite complicated and formerly all operated upon mechanical principles. NDRC sponsored the development of the first electrical director for fire-control use. This fire-control problem is difficult enough for an antiaircraft battery firing from the ground against fast-flying aircraft. It becomes more difficult if the firing occurs from a moving (and rolling) ship and doubly difficult if the combat is between two maneuvering aircraft. In these latter cases the equipment must make allowance for its own velocity as well as that of the target. This may even result in a situation where the gunner firing from one airplane at another must aim behind instead of in front of his target. Many of the new fire-control devices saw combat use. Studies of

certain existing devices produced better methods of using them.

In conventional antiaircraft fire a mechanical time fuze is set to explode the shell at the computed time when the shell will be in the correct position to damage its target. Errors in this computation and in the operation of the time fuze loom large in limiting the effectiveness of fire. An "influence" fuze to be operated in some way by the presence of the target would obviously be a great advance. Photoelectric, radio, and other devices were proposed. Photoelectric and radio models operated satisfactorily and eventually effort was concentrated on the radio form because of its general applicability. It found embodiment in two classes of weapons — in bombs, rockets, and other nonrotating projectiles and in shells. After initial work common to the two types — for nonrotating and for rotating projectiles — the two projects were administratively separated. Shell fuzes reached combat early in 1943. They greatly strengthened the antiaircraft defenses of the fleet, and were restricted at first to this application for security reasons. Shell fuzes later proved very effective, along with radar, and improved fire control when used by antiaircraft troops against the buzz-bomb. General use against ground troops was permitted in December 1944. Shells fitted with proximity fuzes were effective that month in helping to stop the last great German drive in the war. Proximity fuzes on rockets and bombs were not used in combat until early in 1945, when a few were used on rockets and rather more on bombs. Air bursts of bombs so fuzed were found useful against enemy antiaircraft defenses.

Guided missiles developed fall into two classes: those bombs which may be carried in conventional bomb racks but which may be deviated in flight by small amounts to convert near-misses into hits, and larger missiles which attain higher maneuverability at the price of more wing structure and its attendant difficulties. One type of guided missile of each class reached combat use, others were under test when the war ended. These were precision weapons which would destroy railroad bridges or pick out ships in a harbor. One of the weapons made important contributions in the liberation of Burma. Guided missiles are of great interest as forerunners of things to come if wars continue.

CHAPTER II

ADMINISTRATIVE ORGANIZATION

THE NATIONAL DEFENSE RESEARCH COMMITtee was established by order of the Council of National Defense on June 27, 1940. A year later it was merged into the newly formed Office of Scientific Research and Development, which also included the Committee on Medical Research (CMR). The original pattern of organization within NDRC was followed until December 1942. By that time the work had expanded so much as to require reorganization.

The original Committee consisted of Vannevar Bush of the Carnegie Institution of Washington, Chairman, R. C. Tolman of the California Institute of Technology, J. B. Conant of Harvard University, F. B. Jewett of the Bell Telephone Laboratories, Inc., K. T. Compton of the Massachusetts Institute of Technology, Conway Coe, the Commissioner of Patents, and one officer each designated by the War and Navy Departments. These officers were initially Major General G. B. Strong and Rear Admiral H. G. Bowen. The four members first named after the Chairman of NDRC each undertook chairmanship of a Division and subsequently organized a number of Sections. Only those Divisions will be discussed which were involved in the development of those types of weapons for air warfare which are the subject of this volume. Division A, under Tolman, was concerned with Armor and Ordnance. C. C. Lauritsen of the California Institute of Technology was Vice-Chairman of this Division.

In August 1940, Section T was organized under the chairmanship of M. A. Tuve of the Carnegie Institution of Washington, Department of Terrestrial Magnetism, to investigate a number of problems including those relating to proximity fuzes. Section T operated initially through a contract with the Department of Terrestrial Magnetism of the Carnegie Institution. A number of other contracts followed this, including an arrangement for transfer of funds to the National Bureau of Standards for other work on proximity fuzes and certain additional subjects. Section E was organized in December 1940 under the chairmanship

of Alexander Ellett of the State University of Iowa. Its membership included L. J. Briggs, Director of the National Bureau of Standards, W. D. Coolidge of the General Electric Company, and H. L. Dryden of the Bureau of Standards who served as Vice-Chairman of the Section. Shortly thereafter the work on proximity fuzes was divided between Section T and Section E. Section T was given responsibility for fuzes in rotating projectiles and Section E for other applications of proximity fuzes. Early work on guided missiles of the glide-bomb type also came under the jurisdiction of Section E. When the work of Section T came to involve large expenditures for pilot production of fuzes, it was transferred from NDRC jurisdiction but operated within the OSRD framework on funds transferred from the Navy for the purpose.

Division D, under the chairmanship of K. T. Compton, had jurisdiction over radar, fire control, and instruments. The Sections of Division D were numbered (whereas those in Division A were designated by the initial letter of the name of the Section Chairman). Section D-2 took charge of fire control under the chairmanship of Warren Weaver of the Rockefeller Foundation, with Thornton C. Fry of the Bell Telephone Laboratories and S. H. Caldwell of the Massachusetts Institute of Technology as initial members and E. J. Poitras of the California Institute of Technology as Technical Aide. D-3, the Instrument Section, under the chairmanship of G. R. Harrison of M.I.T., and including as members T. Dunham of the Mount Wilson Observatory of the Carnegie Institution of Washington, E. A. Eckhardt of the Gulf Research and Development Company, L. O. Grondahl of the Union Switch and Signal Company, T. J. Johnson of the Bartol Research Foundation of the Franklin Institute, and P. E. Klopsteg of the Central Scientific Company, attacked a wide variety of problems including that of high-angle dirigible bomb. Section D-1 (Radar), under the chairmanship of A. L. Loomis, established the Radiation Laboratory of M.I.T. Here many radar problems were investigated and in due course work on a radar-homing missile was initiated.

With the organization of OSRD, Bush became its Director and was succeeded by Conant as Chairman of NDRC. Roger Adams of the University of Illinois joined the Committee to replace Conant as Chairman of Division C.

The total expenditure of NDRC in all fields for the fiscal year 1940–

41 was somewhat over $6,000,000. In the next fiscal year OSRD spent around $40,000,000 including certain amounts for medical research and for the atomic-bomb project before the latter was taken over by the Manhattan District. In the third year the total appropriation for the whole organization was one-hundred-forty-odd million dollars, and considerable additional funds were transferred from the Army and Navy for specific projects. This growth naturally led to a general administrative reorganization which took place in December 1942. The Committee now served as a sort of Board of Directors to review the work of the new Divisions and to recommend projects to the Director of OSRD. Immediate responsibility was placed in the hands of the Chiefs of eighteen (and later nineteen) new Divisions and two Panels. Many of the old Sections became Divisions without change of the scope of their work, but in other cases the work on a particular subject which had grown up in two or more of the old Divisions and in various Sections was regrouped into new Divisions. Section D-2 became Division 7 with few changes, except that Harold L. Hazen of M.I.T. became Chief of this Division and Warren Weaver, the former Chairman of D-2, became Chief of the Applied Mathematics Panel of NDRC. Proximity fuze work of Section E was transferred to Division 4 with Ellett as Chief of the Division. The title of Division 4 was Ordnance Accessories. The work on guided missiles had been scattered among Sections E, D-1, and D-3. This work was now grouped together in Division 5 under the cover title of New Missiles. Harold B. Richmond of the General Radio Company became Chief of this new Division and was later succeeded by Hugh H. Spencer of the New England Power Company.

More organizational details of the individual Divisions or the earlier Sections will appear from time to time in the narrative, but this much has been given in one place to set the background for the way in which the work was handled.

The early Sections and the later Divisions operated in a wide variety of ways. In some cases, the Section set up by contract a Central Laboratory to which broad authority was delegated. In other cases, the sectional or divisional personnel exercised considerable technical as well as administrative supervision over the work being done in a number of separate but co-ordinated contracts. The form of organization of OSRD permitted this diversity and could take advantage of the merits of each

system. In some cases, the service of the NDRC group was largely of a critical or advisory nature. In other cases, "crash" programs were undertaken leading to large pilot production under OSRD supervision but with funds transferred from the Services.

Considerable compartmentalization took place throughout the work as a security measure, and liaison between Sections and between Divisions required formal authorization for each step. In retrospect, some of these barriers seemed to have retarded the work but they were a valuable security provision. Furthermore, they helped break down initial fears on the part of the military authorities that a civilian organization could not keep security. It is interesting to note that a similar situation existed among various parts of each of the Services and between corresponding parts of the two Services. Channels certainly existed for the transfer of appropriate technical information from the Army to the Navy and vice versa, but many of these channels had become clogged. A number of NDRC Sections and Divisions provided useful informal contacts between their Army and Navy liaison officers. Service Liaison Officers reciprocated by providing connections between NDRC Divisions.

The experiment of a civilian agency responsible for military research was a new idea. Both the civilian personnel and the members of the Army and Navy who dealt with them had to learn to understand each other. There were, naturally, initial difficulties, a few of which persisted for some time. By and large, the relations of NDRC with the Army and Navy were very cordial. It will not minimize this satisfactory relationship to mention exceptions to it from time to time in the narrative. The increasingly technical nature of war gives the greatest importance to cordial co-operation between military personnel and civilian scientists. Each has a role to play and each must depend upon the other. Any discussion of the difficulties of this relationship is put forward in a constructive spirit in the hope that a frank understanding of some of these difficulties is the best way to eliminate them.

CHAPTER III

INITIAL ACTIVITY IN FIRE CONTROL

At a preliminary meeting in Washington, D.C., July 3, 1940, Warren Weaver undertook his duties as Chairman of Section 2 of Division D, NDRC, with the assignment of organizing and leading the work of NDRC in the field of fire control.

On July 15, 1940, Weaver invited Thornton C. Fry and Samuel H. Caldwell to meet with him for a discussion of the organization of Section D-2. Both Fry and Caldwell accepted appointment as Members of the Section and Edward J. Poitras accepted the post of Technical Aide. Thus constituted, and after some delay for personal clearances of these individuals, Section D-2 started operation with a meeting in Hanover, N.H., September 12, 1940.

Weaver had made a brief preliminary investigation of the status of fire control in both the Army and the Navy. From his own observations and discussions with officers in both Services, he had been particularly impressed with the need for improved equipment for the control of heavy antiaircraft weapons, especially for the use of the Army. It was hence appropriate that the first step taken by the Section was a visit to the Coast Artillery Board at Fort Monroe, Virginia, the agency at that time responsible for the characteristics and service testing of Army antiaircraft fire-control equipment.

The written records of that visit on October 3, 1940, reflect to a considerable degree the hearty and enthusiastic reception given by the Coast Artillery Board to the fire-control neophytes of Section D-2. It was not merely a matter of being friendly and hospitable. From the outset there was established a sense of partnership and mutual respect which was to place this Army-NDRC relationship among the most fruitful and most pleasant of the entire war period.

The Coast Artillery Board was fortunate in having as its President Colonel W. S. Bowen, an officer who had integrated a rich and varied military experience into a progressive leadership. Colonel Bowen had the outstanding gift of the ability to use the lessons of history in inter-

preting the needs of the present and the future. He particularly welcomed the efforts of Section D-2 in carrying forward activities designed to bring firmer understanding of the basic problems of fire control, but he counseled the Section to keep its sights raised and to use new knowledge to plan beyond the possible.

With its first visit to Fort Monroe, Section D-2 embarked upon a course which was to take its members, and those who joined them later, into every corner of America and abroad to war zones in the search for superiority over our enemies. In retrospect these were strange times, for America had no declared enemies and not much fear of its potential ones. A few voices were raised in unpopular warnings that the Nazi conquest of France and the Lowlands and the air attack on Britain had meaning for us. A smaller few even ventured to say that we should be more concerned about the behavior of Japan.

There was nothing about a background of science which would endow its practitioners with the gift of prophecy. Socially and politically scientists as a group had no advantage over other men, and they did not begin their mobilization of 1940 from any smug feeling of special insight. There was, however, a numerical quality about the American military scene at that time which warned of inadequacies measured in orders of magnitude. There was not enough of anything and what there was available was of inadequate quality. And while there was no certainty that we would soon be involved in conflict, the numbers which represented the quantity and quality of our equipment were not of the sort which warranted confidence in our ability to undertake even a minimum of home defense.

The narrative which follows does not adhere to a uniform time scale, but attempts to reflect the thinking of Section D-2 and how that thinking was converted into action. It pauses now and then to elaborate on the background of a particular action, and leaps ahead occasionally in order to emphasize the rapid pace of events. It will be found, however, that much of the story is geared to the altitude of the sun. This comes about only because the writer found it comfortable to cast his thoughts in units of time represented by spring, summer, fall and winter — and has no other significance.

In its initial explorations in the field of fire control Section D-2 soon confirmed its general impression that there were many agencies and

many individuals concerned with the subject. Within the limits imposed by meager budgets both the Army and the Navy had endeavored to maintain progress in devising improved methods and equipment. This was supplemented by investigations carried out in private industry and by the inventive efforts of individuals. But the total of these efforts was much too little, and although fire control was a long-established subject, it was by no means a well-established one.

During that fall of 1940 we watched with anxiety the climactic attempt of the German Air Force to bring final defeat to Britain. And our military liaison officers brought us the disturbing news that Mr. Churchill's eloquent tribute to the RAF was almost an understatement, for the performance of antiaircraft artillery in the Battle of Britain had been both expensive and ineffective.

With this background Section D-2 elected to concentrate its major initial activity in the field of ground-to-air fire control. From an examination of both Army and Navy equipment it was apparent that the Army offered the greater opportunity for accomplishment. This conclusion was derived partly from a technical appraisal of the situation, but even more from the fact that by and large the Army was less satisfied than the Navy with the quality of its own equipment, and more disposed to welcome the co-operative efforts of Section D-2 in carrying out new developments.

The first program of the Section contained two broad classes of work. First, there was a study of fundamental problems including (a) methods for measuring and analyzing errors in antiaircraft computers, (b) servomechanisms, (c) essential functions of computers and computer components, (d) the probability of success in antiaircraft operations and the factors which influence it. The second major item was a theoretical and experimental attack on the specific development of a more effective fire-control system for heavy antiaircraft weapons.

A completely orderly and systematic execution of the Section program would have proceeded by first studying the fundamental problems in order to acquire the experience and factual data required either to formulate or to satisfy the requirements for an improved antiaircraft director. (Actually the Section's first formal project was for research at the Massachusetts Institute of Technology in the general

field of servomechanisms.) But the exigencies of the situation required the less ideal and more expensive approach of carrying out both aspects of the program simultaneously and in more or less opportunistic fashion. Thus the Section decided to undertake an immediate program of antiaircraft director development.

At about this time M. J. Kelly, Director of Research of the Bell Telephone Laboratories, proposed, in rather general terms, a director in which electrical computing elements were to be employed extensively in place of the more conventional mechanical types. This general idea interested the Section for several reasons. First, because electrical computing techniques had not been much exploited in fire-control equipment, it offered an opportunity to supplement rather than to duplicate the work of other agencies. Furthermore it seemed likely that the manufacture of electrical elements might be a relatively simple matter in comparison with the manufacture of the precision elements of a mechanical director. Second, the inherently greater flexibility of electrical systems in comparison with mechanical systems rendered the whole idea of plunging into immediate director development more palatable. It offered the possibility of keeping the final design "unfrozen," in many particulars at least, for a longer period of time. Third, the actual proposal arose from the initiative of a scientific organization of outstanding competence and resourcefulness; it was "born with a silver spoon in its mouth" from the standpoint of getting strong scientific and engineering support.

During the first visit to the Coast Artillery Board, Section D-2 explored the electrical-director situation further by seeking the reaction of the Board to the general proposal, and in particular sought for any opinion available concerning the probable success of electrical computing elements under the conditions of field use and maintenance.

It was learned that while Army opinion on the subject was divided, the Coast Artillery Board could see no reason for not using electrical methods. The established mechanical methods had the usual attraction of being familiar friends, but it was pointed out that the Army also used radio equipment and was at that time contemplating vast expansion in the even more recondite field of radar. All in all, the electrics and electronics of an antiaircraft computer promised to be quite docile and commonplace compared with those of other Army equipment.

Direct and detailed examination of the proposed electrical director was undertaken at a visit to the Bell Telephone Laboratories October 24, 1940. Much was clarified at this meeting for it was apparent that the group under Kelly — Fletcher, Wente, Lovell, and Parkinson — had put much creative thought on the subject and had already formulated sound proposals for many of the key problems. The attitude of these men in the presentation of the proposal and in dealing with questions raised, the attitude of the administrative officials of the Bell Telephone Laboratories in the matter of providing personnel and facilities for such a project, and the inherent technical merit of the proposed director itself; all these combined to bring Section D-2 to the conclusion that the project was worthy of immediate support even though its ultimate success was by no means inevitable. Subsequent action was swift, and active work on this, the Section's second project, started within less than a month.

These were the simple beginnings of the T-10 director, which ultimately became the famed M-9 director. The trinity of SCR-584 radar, M-9 director, and the proximity fuze (all NDRC or OSRD developments) was in a few years to become a devastating answer to the buzz-bomb and a power to be feared by the enemy in the air.

The idea of developing the electrical director was proposed by the Bell Telephone Laboratories to the Army some time before Section D-2 came into existence. It was stated earlier in this account that opinion in the Army was divided regarding the merits of the proposal, and it was known that some of the cognizant officers favored the proposal. Because the proposal had initially been made to the Army, Section D-2 reported, through the Ordnance Department liaison officer with NDRC, its conclusion that the project should be undertaken. It offered its services either by co-operating on an Ordnance Department project or by initiating direct action through an NDRC contract. When it was decided that the work should be done under NDRC auspices, Section D-2 then requested that appropriate officers be delegated to attend a conference to set the military specifications for the first electrical director. These specifications were agreed upon and turned over to the Bell Telephone Laboratories personnel for execution in the design.

From that time until the accepted director was turned over to the Army for procurement and further refinement based on operational

experience, the project was the responsibility of Section D-2. During that period, the Section enjoyed the enthusiastic co-operation of many officers. Various Army agencies rendered assistance in supplying standard and special equipment needed. Particularly helpful were the Coast Artillery Board, under Colonel Bowen; the Fire-Control Design Section at the Frankford Arsenal, first under Colonel Gordon M. Wells and later under Colonel G. W. Trichel; and the Antiaircraft Artillery Board under Colonel R. W. Crichlow after this group was organized as an agency distinct from the Coast Artillery Board. It should be noted also that Major General J. A. Green was from the beginning a staunch friend of the project.

The director was tested jointly by the Coast Artillery Board and by Section D-2 at Fort Monroe, and test reports were rendered independently through both Army and NDRC channels (the two reports were written for different purposes and with different points of view).

Returning to the general activity of the Section, the remainder of the year 1940 found its members engaged in a strenuous effort to absorb the lore of fire control. A variety of projects were undertaken at California Institute of Technology, Massachusetts Institute of Technology, and Princeton University to investigate general problems in optics, theory of prediction, and the theory of computer mechanisms.

At the invitation of the Coast Artillery Board the Section witnessed a demonstration by a British crew of the Kerrison predictor (British Predictor No. 3) controlling a Bofors 40-mm. gun. After a detailed examination of the equipment, an advisory set of recommendations was offered regarding its merits for American use. This was the first of a long series of subjects in which the Section and its successor, Division 7, was asked for advisory reports by various Army and Navy groups.

Meanwhile there were emerging two problems of obviously long-term character, but yet so vital to progress in fire control that they demanded immediate and strenuous efforts to obtain solutions. The first of these was the lack of adequate instruments and procedures for the measurement, analysis, and assessment of the performances of fire-control components and systems. It was literally impossible to make a decision regarding any fire-control equipment from an appreciation of realistic, quantitative data. The conventional test of an anti-

aircraft system by firing at a towed target was useless as a means of scoring the technical performance of the equipment. It was worse than useless as a means of indicating to a project engineer whether his equipment was getting better or worse, particularly if the degree of change was small or moderate. The result was completely obscured by a host of uncontrolled and uncontrollable factors, including the behavior of the target, variations in the gun and ammunition, training and skill of the operating personnel, the fragmentation pattern of the shell, errors in the data supplied to the computer, changes in atmospheric conditions, and the frequent loss of the target itself. One might as well have attempted to measure the tractive effort of a locomotive by counting how many freight cars it could pull, without regard to whether they were loaded or empty, how much they weighed, whether the track was level or on a grade, or for that matter, whether the cars had wheels or sled runners.

The second fundamental problem was closely associated with, and in a sense contained within, the first problem. It was the problem of understanding and treating advantageously the interaction between the man and the machine.

In the most primitive method for fire control — using tracer bullets — a man endeavored to track a target in two co-ordinates, sense the time of flight of a bullet by observing when a tracer seemed to be at target range, combine these observations into an estimated lead, superpose the lead on his tracking, decide when it was advantageous to fire, observe what he thought was his error and correct it — and do all these things smoothly.

In the more elaborate systems used with heavy weapons the whole job was broken down into component tasks, but each task demanded a higher quality of performance. Although an individual might be allotted the apparently simple task of tracking the target in azimuth,[1] the computer attempted to extract from his performance information about the azimuth rate of the target. Smoothness of tracking, in addition to average error, became a criterion of performance. The "simple" task of transferring a shell from the fuze setter to the breech of the gun was a

[1] The angular position of an airplane target in the sky is defined by two angles. One, called the azimuth, fixes the direction of the line on the ground from the observer to a point directly under the airplane. The other angle, called the angular height or elevation, is that between this horizontal line and the line of sight from the observer to the airplane.

source of anguish to the computer design engineer who had to count on a constant dead-time allowance for this operation.

But the top problem of all in this category was that of operating the stereoscopic height finder, or range finder. This man-machine link was of critical importance because of the fact that a continuous knowledge of target range was indispensable in the prediction of the target's future position. Errors in this operation were by far the largest source of over-all system error. A reduction of these errors hence offered a maximum opportunity for improving the performance of existing antiaircraft systems.

It was evident that many aspects of both problems would require the use of military equipment and personnel under carefully controlled experimental conditions. Furthermore it was considered desirable to conduct a program aimed at the solution of these problems with direct military participation so that the techniques and procedures developed would be understood and used with confidence beyond the period of NDRC sponsorship.

The initial implementation of the Section's program of fire-control measurement and man-machine studies was through a contract with Princton University. With the co-operation of the Coast Artillery Board and the Coast Artillery School the contractor built, equipped, and operated a laboratory at Fort Monroe, Virginia. Although this project was located and operated on an Army post, it was not restricted to work on Army problems. As a matter of policy Section D-2 directed the project through the agency of a Steering Committee on which both Army and Navy officers served, together with a selected group of civilian scientific consultants, under the chairmanship of Fry.

During its existence the Princeton Laboratory explored far and wide in the search for the sources of fire-control errors, and developed much of the analysis and assessment technique now used by the Antiaircraft Board at Fort Bliss, Texas. About seventeen formal Reports to the Services were issued from this project during 1941 and 1942. Eventually other aspects of the work required expansion of the facilities through contracts with half a dozen university and industrial laboratories.

Much of the work in the Princeton Laboratory was concerned with the instrumental and the psychophysiological errors encountered in the use of optical height finders and range finders. In the light of the pres-

ent status of radar, not only as a range finder, but also as a sentinel and as an automatic tracking device, it may seem strange that as late as December 1940 an extensive study of optical height or range finders was planned and undertaken. To some extent this is merely a commentary on the phenomenally rapid growth and development of radar within a five-year period. When the Princeton Laboratory was organized, the optical range and height finder was a necessity and had no real competition in terms of equipment "in being" which met military requirements for supplying fire-control data. It was considered essential during the interim period of radar development to bring existing production instruments to the highest possible level of performance. Furthermore, elementary considerations of prudence demanded that the situation be strongly hedged against two possibilities: (a) that unforeseen development, production, or training difficulties might seriously delay the application of radar as a means for obtaining fire-control data under the highly mobile conditions of field use; (b) that the enemy might develop effective countermeasures. (Section D-2 was a vigorous advocate of a strong program of radar countermeasure development on our own part, particularly for its value in guiding policy on the structure of fire-control systems.)

But Section D-2 did not rest with a program of mere watchful waiting with respect to radar. In January 1941 a request was made to Section D-1, NDRC, for the development of radar equipment to provide range data for antiaircraft fire-control systems. At that time Britain was in the midst of the agony of night bombing. The Radiation Laboratory was a vigorous organization but still a small one. Its energies were directed, under top priorities, toward the development of night-fighter interception equipment. After a survey of the possibilities Section D-1 recommended that Section D-2 undertake to develop a radar range finder under separate contract with another laboratory. This development assignment was accepted by the Bell Telephone Laboratories. The equipment produced under this D-2 contract was standardized by the Army and subsequently was procured under the designation SCR-547 range finder. The performance of the first model of the equipment (then known as Mickey Mouse) tested at Fort Monroe was such that soon certain high officers of the Army were discussing the abandonment of its optical range-finder procurement — to the consternation of Section

D-2, whose members still regarded optical instruments as a necessary hedge against the possibility that the enemy might find an effective radar countermeasure.

In the field of airborne fire control, the early activity of Section D-2 was predominantly exploratory. In retrospect it is clear that the discouragingly slow progress of development in airborne fire-control equipment was primarily rooted in a lack of quantitative information concerning the true nature of the problem. It was the same old problem of measurement, made still more difficult when the origin of co-ordinates had to be carried all over "the wild blue yonder."

Despite these uncertainties and difficulties some progress was made in understanding the kinematics of aerial combat and the nature of the mechanisms then in use and proposed. In particular the type of mechanism usually designated as a lead-computing sight was given considerable attention. Some mathematical work was done on the subject at the University of Wisconsin but the bulk of the initial analysis was done within the Section. The Section also accepted a request from the Armament Laboratory of Wright Field to develop local and remote hydraulic controls for special .50-caliber gun installations. This work was carried out under contract with the United Shoe Machinery Corporation.

Early in 1941 the attack on the antiaircraft problem was intensified by the negotiation of another contract with the Bell Telephone Laboratories for fundamental studies on director systems. This was Project 11 on the Section D-2 list and was eventually to become one of the most fruitful sources of incentive and basic understanding in the antiaircraft field. The art of the communications engineer was peculiarly applicable to the feed-back elements of the systems used, and to the frequency distributions encountered in the input data.

Meanwhile the work in the Princeton Laboratory at Fort Monroe was gathering scope and momentum and further facilities were urgently needed. Tufts College undertook a contract calling for studies of psychological and physiological factors in fire control, and as part of a contract at the Iowa State College certain studies in the physiology of tracking were undertaken.

During this period a contract was negotiated with the Eastman Kodak Company for the development of a new type of optical range

finder proposed by Joseph Mihalyi. But the men of this Company displayed such energetic interest in the work of the Section that within a few months the Company undertook a broad assignment in the development of optical, photographic, and computing instruments and techniques for use in all phases of the Section's work. This narrative and the succeeding parts which deal with the activities of Division 7 will refer frequently to the work of the Eastman Kodak Company, particularly under Project 17. From the outset it was a tower of strength to the operations at Fort Monroe and the Company continued its vigorous co-operation until the last shot was fired.

In March 1941 the war pouring from the night skies of England suddenly became a matter of grave concern to the Section when Weaver and Poitras were designated to conduct a mission in England in connection with the work of the newly established London office of NDRC. Except for an upset in an acrobatic English automobile, which inflicted rather painful injuries on Weaver, they survived six tragic weeks in the heart of the blitz. It was an unparalleled opportunity for the fire-control man, and Weaver and Poitras made maximum use of this full-scale laboratory. In addition to the many specific matters on which they reported, they returned with confirmation of the soundness of the Section's basic program, but also with much that was to serve as guidance and incentive for the program to come.

It was time in the spring of 1941 to take stock and consolidate the position of the Section for more effective action in the next phases of development. Up until this time the Section had more or less acted as a unit in conducting its business. This was rather necessary during the first six months, for much of the business of the Section was to learn its business. By working intensively and with close mutual contact the benefits of multiple observations accrued rapidly. Many officers in the Services had by this time come to rely upon the members of the Section, individually and collectively, for pertinent advisory assistance, rendered with a degree of objectivity which was a prime asset of the Section. In these relations, the Section came to occupy for a time the peculiar position of being the agency through which the Navy came to learn things it should know about the Army fire-control program, and vice versa. The phenomenon was one of osmosis rather than of communication, and the obvious advantages which resulted led, in the course of time, to

the establishment of direct inter-Service exchange of information and data.

The laboratory which had been established at Fort Monroe was a focal point for this sort of informal exchange of information. It soon became commonplace to see officers of both Services in conference there, and British uniforms were not a novelty. The Section sought to cultivate this spirit, and is proud of its success in doing so. It is equally conscious of the fact that it could not have succeeded if it had not been met fully halfway by an unusual group of officers, among whom Colonel Bowen, Major General Green, Brigadier General F. S. Clark, and Vice-Admiral W. A. Lee, Jr., deserve special mention.

The Section was spreading its manpower rather thinly and a search was begun for additional help. Within the next few months considerable recruiting was done. D. J. Stewart of the Barber-Colman Company became a member of the Section and eventually assumed prime responsibility for antiaircraft developments other than problems of optical range finders. The need for supplementing the voluntary services of the Section members was recognized in the appointments of three full-time Technical Aides. Samuel W. Fernberger of the University of Pennsylvania undertook service with Fry, specializing on the psychological aspects of fire-control problems. John B. Russell of Columbia University served with Caldwell in two fields: in the application of radar techniques and equipment to fire control, and in plane-to-plane fire control. George R. Stibitz of the Bell Telephone Laboratories joined with Stewart in work on the theory, design, and test of directors.

During this period Poitras carried principal responsibility in the field of servomechanisms, with secondary responsibility for antiaircraft directors. Until he was joined, late in 1941, by M. H. Trytten, he organized and operated the Washington office of the Section, a task of key importance because of the many necessary and close contacts required with Army, Navy, and other Government agencies.

Also during this period a considerable activity evolved in the mathematical analysis of fire-control problems. The work included the theoretical analysis of predicting mechanisms, the analysis of the dynamical behavior of various types of sights, the analysis of devices for smoothing input data, and a wide variety of probability studies. For the most part, Weaver supervised this work personally, in addition to his general

duties as Chairman of the Section. He later had the assistance of H. Germond in this activity.

Additional problems and additional opportunities came to the attention of the Section. Notable among the problems was that of further implementing the measurement of antiaircraft director performance. At Fort Monroe, considerable progress had been made in setting up elaborate synchronized systems of motion-picture cameras and theodolites for simultaneous measurements of target position, director input, director output, and computed rates. This system required extensive calculation to reduce the measurements to interpretable figures, and while it was a necessary feature of the final testing of a complete system, there were many preliminary tests for which a simpler and more rapid method was desired. The big handicap of the towed-target method was that the path and speed of the target could not be predetermined, or even repeated. Hence each test had to be treated as a special case and calculated in detail. It was also serious that the speed of a towed target was unrealistically low.

The idea of using an artificial target was not new, and this expedient was set up at Fort Monroe in the form of a "running rabbit" — a target mounted on wires and moved in predetermined fashion by a motor drive. Because of the short range at which the artificial target was used it was necessary to allow for the parallax errors caused by the separation between the azimuth-and-elevation-tracking telescopes.

While the artificial target brought the testing problem down to smaller dimensions and under better control, it did not change the fundamental problem of having to compute the performance of the director and its operating personnel from measurements made on the target motion and the input and output of the director. And it should be emphasized that whatever errors occurred, under either system of test, were a combination of the errors of the human operators and the errors of the mechanism. There was at this time no means available for testing the mechanism alone under conditions which correctly reproduced the dynamics of normal operation, although this need had been suggested by Frankford Arsenal.

This problem was attacked under a development contract with the Barber-Colman Company. The device that resulted from their work was known as the Dynamic Tester. For each target course the Tester

contained three input cams which gave the azimuth, elevation, and range (or height) of the imaginary target, and three output cams which gave the correct gun azimuth, gun elevation, and fuze time required to "hit" the target at any point. Servomechanisms were provided to drive the input members of the director under test in accordance with the target motion provided by the Tester. Thus the data furnished to the director were, to a very close approximation, devoid of tracking error. The gun and fuze orders which came from the director were then transmitted to receiving units in the Tester, where they were compared with the correct data contained in the output cams of the Tester. Each director output was subtracted mechanically from the correct value of the corresponding quantity from the Tester and the resultant error was automatically recorded on a chart.

Thus there became available a means for putting a director through a predetermined problem and obtaining a complete history of its error under dynamic conditions without using any sort of target and with no auxiliary computation required in order to obtain the errors. For the first time it was possible to separate the human errors from those of the mechanism. This type of result has a profound effect on the training program for antiaircraft crews, for it shows exactly what level of proficiency is reasonable in terms of a measurement of over-all performance. But the influence on the technical development and design of directors is even more important. Because the test is fully reproducible, it becomes possible to explore the sources of error within the director itself, by comparing the results obtained before and after modifications of the structure taken one at a time.

The Dynamic Tester was a rather devastating device. It had no respect for the opinions of experts, including those within Section D-2, and it gave no credit for lucky hits. It was never intended to replace firing tests and it never did so. But it did replace the firing tests which had been necessary to determine the dynamic accuracy of the director, and it permitted the more profitable use of firing tests for studying the field qualities of the director and associated equipment. Its development was continued throughout 1941 and 1942, and when the work of Section D-2 passed to Division 7 at the end of 1942 a project for the development of an even more versatile Dynamic Tester was ready for action.

By the summer of 1941 activity in airborne fire control began to increase, particularly through the establishment of working contacts with appropriate officers of the Navy Bureau of Ordnance and Bureau of Aeronautics. The process of gaining experience began all over again, this time in the air. Many devices were under development, largely in industrial laboratories, but there was no evidence of a co-ordinated program based on adequate fundamental data and theoretical understanding. This is not to be taken as a statement critical of the men who initiated and pursued the various projects under way. It is merely a hindsight summary of the state of affairs brought about by an emergency effort to do something which had been too long neglected and unsupported by the Congress in budget after budget.

A significant step was taken in August 1941 when the Section distributed a Report to the Services on the fundamental dynamics of the lead-computing sight. Although the material was applicable to other fire-control situations, this analysis was primarily inspired by developments in the field of aerial-gunnery devices and was the Section's reaction to the general scarcity of adequate theoretical material. It provoked much healthy discussion and debate and eventually became one of the classic papers on the subject.

During the spring of 1941 continuous liaison had been maintained with Section D-1 of NDRC, particularly with the view of obtaining, as soon as other requirements permitted it, an intensive development of fire-control radar. By the time summer arrived the expansion of the Radiation Laboratory and the degree of success achieved on the night-fighter problem had enabled Section D-1 to give more attention to the fire-control problem. Also by this time the radar range finder had been developed for Section D-2 by the Bell Telephone Laboratories. Its favorable reception by the Army was an incentive to develop a universal fire-control radar which would provide angular as well as range data and be useful to some extent for searching.

Because of the early preoccupation of the Radiation Laboratory with the aircraft interception problem its contacts in the fire-control field were very limited, and an expedited program of orientation and indoctrination was considered advisable. K. T. Compton provided a mechanism for this purpose by setting up a Joint Committee from

Sections D-1 and D-2, known as Section D-1.5. This Committee consisted of Caldwell (Chairman) and Fry from Section D-2, E. L. Bowles from Section D-1, and L. N. Ridenour and I. A. Getting from the Radiation Laboratory. Its major accomplishment was the first general survey and report of all radar developments under way or completed in all American and Canadian agencies. The Army and Navy and the Canadian Government granted broad authorizations to the survey group and assigned liaison officers to accompany them in this task.

Although it was incidental to the purpose for which the D-1.5 Committee was formed the survey actually accomplished most in helping to launch the Radiation Laboratory program of fire-control radar development. It also was of invaluable benefit to the members of Section D-2 who participated. Having accomplished its purpose, the D-1.5 Committee was discharged in April 1942.

As the summer of 1941 passed, the tempo of activity accelerated. The theoretical studies at the Bell Telephone Laboratories were beginning to show the promise of even more important developments possible in electrical directors. Also two projects were under way for the development of precision data-transmission systems for coast defense batteries. Through Stewart, the Barber-Colman Company undertook (at its own expense) the development of a servomechanism for the 90-mm. gun, and the development of a mechanical director based on the memory-point method of prediction. Reverse Lend-Lease was operating to send many British scientific visitors to the United States, and Section D-2 took vigorous advantage of the opportunity to learn at first hand some of the needs of a shooting war.

Thus the Section came to the end of its first year of operation, not much the worse for wear, with a fairly objective pride in its progress, but with a profound conviction that it still had much to do and much room for improvement.

October of 1941 brought a variety of new activity. The Dynamic Tester was almost ready for use and it faced a heavy schedule of applications. The SCR-547 range finder was going into production and the Section worked closely with the Signal Corps and Ordnance Department on problems thus arising. At this time the Section was asked to work on problems of rocket fire control and started exploring ideas in

that field. The submarine problem had become very acute and the Section was asked to devise something to help in the aiming of depth charges dropped or projected from destroyers.

Aerial gunnery took a new turn when the Army decided to attempt to develop a fire-control system for very heavy bombardment planes in which radar would provide all the target data required. Representatives of Section D-2 at the planning conference pointed out that an entirely new type of computer would be required for this application. This development was undertaken by the General Electric Company under Air Forces contract, with Section D-2 acting in an advisory capacity. Although the all-radar system was never used, the development started for this purpose at the General Electric Company became the source of the computer ultimately used in the B-29 airplane.

The analytic studies on basic director design, which had been undertaken by contract with the Bell Telephone Laboratories, finally brought out a new proposal for a director which appeared to have superiority over the M-9 director. In November 1941 the Section sponsored another contract with the Bell Telephone Laboratories to design and build the new type. This director, known as the T-15, had an interesting history which will appear later.

In the meantime Army pressure for a production design of the M-9 (then known as the T-10) director was mounting. There was difficulty in attaining satisfactory production levels of the existing mechanical types of directors, and there was a strong feeling that even if the M-9 performed no better than the mechanical directors, it was worth producing for the sake of the enlarged procurement facilities thus made available.

Events moved swiftly indeed. Within the one month of November 1941, the electrical director reached laboratory completion (a phenomenal record of developing and building the director from scratch in one year), the Army made the decision to produce it in quantity, and November 29, 1941, the director arrived at Fort Monroe for field tests. In one way the electrical director development was entirely too successful, for it brought with it a hectic and heated series of Army-Navy-NDRC-British conferences to settle conflicting demands for priority in allocation.

The field of light antiaircraft fire control had not been forgotten.

Signal Corps Photo

The SCR-547 Antiaircraft Range-Finding equipment ("Mickey Mouse") in action

Signal Corps Photo

Units of the M-9 electrical gun director system

Signal Corps Photo

The tracker for the M-9 electrical gun director

Section D-2 had endorsed the Kerrison predictor with the qualification that it was considered a stopgap solution. In addition to extensive Section discussions and studies of this problem, experimental work was carried out at the Eastman Kodak Company on the development of optical aids for ranging and correcting range, and modified versions of the Kerrison predictor were developed under contracts with the G. M. Laboratories, Inc., and with the Barber-Colman Company.

The momentum of November carried the Section through the remainder of 1941 with a continued high level of activity in all the fields which have previously been discussed. By this time there were twenty-four projects under way at the Princeton Laboratory at Fort Monroe and the Dynamic Tester had been placed in use. The needs of light antiaircraft equipment had led to the development by the Eastman Kodak Company of a short-base optical range finder, and a contract was set up at the Polaroid Corporation for further development of this type. The design of an airborne, central-station type of gunnery computer was well under way at the General Electric Company, and the Sperry Gyroscope Company undertook to convert a central-station computer, which they had developed for use with optical inputs, into a type to accept radar inputs.

Perhaps the most significant feature of the end of 1941 is that the writer, in a careful search through the diaries and correspondence of the Section, found no reaction to Pearl Harbor, and, in fact, no mention of it. The simple fact was that Section D-2 had gone to war on an all-out basis many months before Pearl Harbor.

A few weeks later, a record does appear of a sort of delayed reaction, and this record is also such a striking example of the level of co-operation achieved with the contractors of the Section that it is quoted below, in full. Briefly, a method for selecting personnel for training as range-finder operators had been developed at the Princeton Laboratory at Fort Monroe. A new class was to be selected at Fort Eustis to receive training at Fort Monroe, but the new method of selection, based on the use of vectograph prints, had not been devised until very late. To get the material ready for the fixed dates of a large-scale Army training program required a race against time in the best Hollywood manner. Dr. Fry's diary description, which appears below, is a record worth preserving.

Extract from Diary of TCF[2] — January 23, 1942

6) The saga of the vectographs is complete. It is also an amazing instance of all-out cooperation and ingenuity in defense effort. . . .

In outline the story is: Saturday, January 3, 1942, Wulfeck exhibited the sample vectograph test; TCF called Stuber asking if EK could produce and mount 100 prints each, of four times as many pictures as were in the sample, and have them at Monroe by Monday, January 12; Stuber promised an answer by 10 A.M. Monday, January 5, and Wulfeck agreed to have on Stuber's desk by that time a telegram giving details of the pictures desired. To appreciate what happened from this point on, it must be realized: (a) that the request was for 4000 vectograph prints — more than had been made in the previous history of the world; (b) that film on which to make them was not available; (c) that only Polaroid could make the film and they only by small-scale processes; (d) that Polaroid did not have acetate base on which to make the film; (e) that *no one* had a work room equipped for making and mounting the prints; (f) that vectographs with disparities as low as 2 units of error were desired, which required very precise workmanship in making the negatives. Probably if these difficulties had been realized at the start, the job would not have been done.

Instead of telegraphing Stuber, TCF and Wulfeck showed up in his office on the morning of Monday, the 5th, and Wulfeck immediately went to work with a photographer named Chick (whom Eastman had already got on the grounds) to lay out the pictures required. The difficulties were overcome as follows: —

Fortunately in the process of making Wulfeck's sample pictures Kodak had made a cute gadget by means of which negatives of sufficient precision could be produced. Eastman sent Land (at Polaroid Corporation) acetate by Tuesday, and he returned the vectograph film by Wednesday morning. (What the story is back of this performance TCF does not yet know.) Meantime, Eastman hired Chick, his staff and his studio, for the week. Fortunately Chick's studio included a huge unused loft. Chick quickly planned a production department on a "straight line" basis, and by Monday afternoon had carpenters, plumbers and electricians at work building it. The quarters had to be dust-free and forced ventilation was required. Curiously enough, though the carpenter did not know this, he showed up on the job with the right sort of ventilating fan, which he had

[2] The following cast of characters will identify the individuals referred to in this episode: —

TCF—T. C. Fry.
Stuber — A. Stuber, Vice-President, Eastman Kodak Co., Rochester, N.Y.
Land — Edwin H. Land, President, Polaroid Corporation, Cambridge, Mass.
Wulfeck — W. H. Wulfeck, of the Psychological Corporation, who had developed the test while stationed at the laboratory at Fort Monroe.

just taken out of a local theater and had not had time to unload. The workmen left the loft Wednesday morning, about 15 minutes before the film arrived from Polaroid.

Then the fun really began. Chick, with his staff, his wife, the carpenter's wife, several Eastman engineers and various in-laws of Chick's (a relative from a neighboring town dropped in Wednesday afternoon for a pleasant chat with Chick's wife, was immediately put to work, and left on Saturday night) started the production line, and by working 18 to 20 hours a day — continuously, not in shifts — got out and mounted the 4000 prints. Half of them went out for Monroe Saturday night, the rest by messenger the following day. And they didn't do a sloppy job. The mounts were neatly marked with rubber stamps to indicate the top, and with numbers for columns and letters for rows.

As a result, the test was used at Eustis on the week of January 12, in selecting the next class for Monroe. It would otherwise not have been possible to try it out until ten weeks later.

TCF is normally a pessimist, and has been particularly pessimistic about this war. But if the spirit shown by Kodak and Chick, and imparted by them to the carpenter and the carpenter's wife, is what Japan gave us at Pearl Harbor, the war is won. You can work a man twenty hours a day by regimentation; but you can't order up by that means the enthusiasm and initiative that produced 4000 vectograph prints in a week, starting from nothing flat. And this crowd is hankering for a chance to do it all over again.

Initial field tests of the M-9 electrical director were completed in the early months of 1942. It was a good job — perhaps not up to the level expected by the most optimistic, but it easily justified the advance work on production engineering initiated by the Ordnance Department. Moreover the entire philosophy and technique of director testing had changed. The tests made on the M-9 were the penetrating sort that provided not only an over-all assessment of quality but also the information on which improvement could be based. While there were many faults to be cured, the important lesson of the M-9 development was that of appreciating the need for a sophisticated approach to the problem of data smoothing. Here the lore of the communications engineer was of paramount importance, for the problem required an understanding of the frequency spectrum nature of the input data and of their errors. This was necessary before undesired frequency components could be removed without losing or changing useful information. Fortunately the electrical director had the required flexibility of structure to permit changes to be made with relative ease, and the information derived

from testing and further experimental work with the laboratory model was incorporated promptly in the production designs.

At the Princeton Laboratory an important stage of progress had been reached in the study of the optical height-finder performance. Extensive experiments and analysis had revealed a major source of error due to the stratification of air within the tube of the height finder. These strata, which caused small but significant refractions, existed under normal conditions as the result of temperature gradients. Various methods were tried for reducing or eliminating the stratification error. The procedure finally used was to improve the seals of the instrument and then replace the air with helium. This was not a simple substitution process. It was necessary to maintain a slight positive pressure in the instrument so that helium would leak out rather than air leak in, but the system had to be tight enough to keep the expenditure of helium at a minimum (not because of cost but to avoid further difficulty of supply and transportation of the heavy helium cylinders. Also provision had to be made for the maintaining of constant pressure during changes of ambient temperature. This required the development of breathing apparatus so designed that the instrument would lose gas as the temperature increased and have the loss replaced from a supply cylinder as the temperature decreased.

The design of the equipment for helium-charging of range finders was carried out by a Section D-2 contract with the American Gas Association Laboratory. The Army adopted both the procedure and the equipment for service use.

At about this time the Eastman Kodak Company worked out an interesting idea for avoiding many of the large and small errors in the range finder. Although first devised for use with a coincidence-type range finder, the principle was later applied to the stereoscopic type. Briefly, the idea — called autocollimation — was to arrange the optical system so that light rays from the reading scale passed through the same portions of air space and through optical elements subject to the same mechanical shifts as the light rays from the target. Once the system was collimated any mechanical or optical shifts thereafter automatically modified the reading scale of the range finder by the amount required to maintain correct calibration.

This principle was applied in various forms in a number of short-

base range finders developed in the course of the Section D-2 program and later was used in the Division 7 program.

Some of the problems of airborne fire control had begun to crystallize by early 1942, and in February Section D-2 set up a contract at the Franklin Institute in Philadelphia to provide personnel and laboratory facilities for experimental work in that general field. From a modest beginning, this project grew rapidly under the rising tempo of air warfare. Its work will be referred to frequently, and especially in the narrative for Division 7.

Initially the Franklin Institute undertook a project requested by the Navy Bureau of Ordnance for the development of a stabilized aerial torpedo director, and a series of studies relating to the ability of gunners to track with lead-computing sights, using various types of aircraft turret controls. The first of these projects led to the Mark 32 torpedo director — a highly air-minded device. It weighed only about one pound, yet it gave the pilot of the torpedo plane a point of aim which took into account the speed, course, and range of the target, the water speed of the torpedo, and the speed and altitude of the plane, and allowed the pilot to approach the release point with any evasive maneuvers he chose to take. This was only the first of a long series of torpedo-director developments which continued throughout the tenure of Section D-2 and Division 7. The work was carried out in close collaboration with the Bureau of Ordnance, and most of the developments were supplied also to the Armament Laboratory at Wright Field for Army Air Forces adaptation. But in spite of a lot of good work, both in the laboratory and in production design, and in spite of diligent efforts on the part of the Bureau of Ordnance, none of these torpedo directors was ever used in combat.

The origin of the aversion to torpedo directors on the part of the combat forces was not entirely clear. At training stations we were told, "A torpedo director is a good device for teaching a flier to use his 'seaman's eye.'" And in combat they appeared to use the "seaman's eye" method — in other words, a more or less educated guess. Probably this arose from a combination of factors. Early torpedo directors were not handy, and torpedoes were so erratic that careful aiming was not worth while. A torpedo attack was a risky operation at its best and tended to become a hit-and-run affair with squadrons dropping fleets

of torpedoes having a spread to overcome the errors of the process. Although these factors were recognized, the discouragement was accepted by the Section as part of the fortunes of war, and torpedo-director development was continued — continued because there was always the possibility of a change of doctrine which would create a sudden demand.

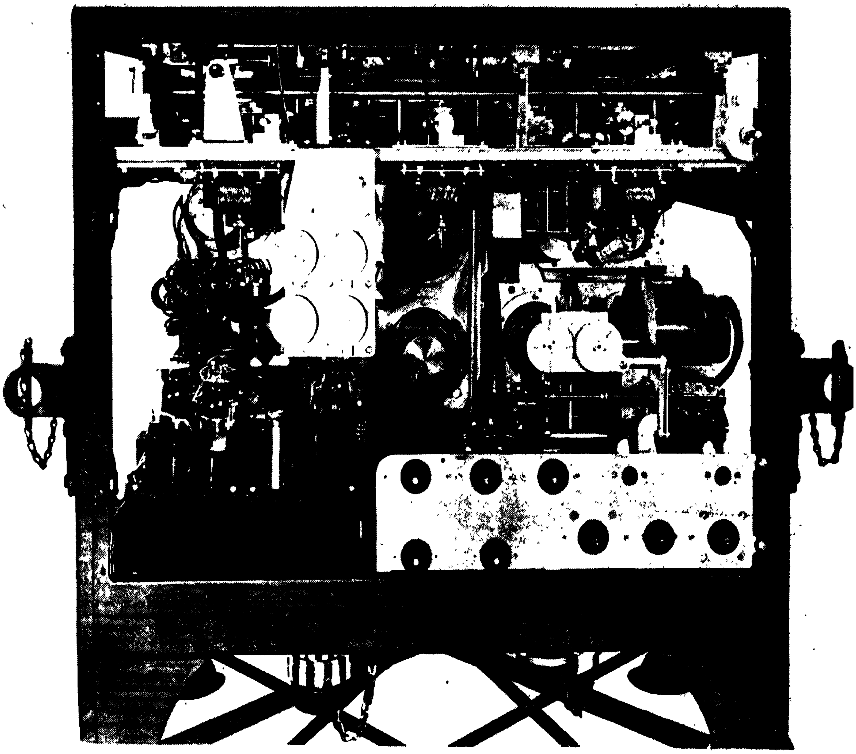
Mention has been made of the tracking studies carried out at the Franklin Institute. Section D-2 initiated this program by starting the construction of the special equipment required. The main body of the work, however, was carried out under Division 7 auspices and will be discussed later.

With the arrival of the spring of 1942, George A. Philbrick joined the Section as a Technical Aide and undertook the principal technical responsibilities for the work at the Franklin Institute. At the Leeds and Northrup Company a pilot model design of the coast defense data-transmission system was started. Dartmouth College contracted to study the effects of fatigue on space perception, and The Foxboro Company started a study of controls and data presentation. The latter two projects arose from the Section's activities at Fort Monroe. The Massachusetts Institute of Technology undertook two projects: the first a report by Norbert Wiener of his mathematical studies on stationary time series, and the second a project under Gordon S. Brown for the improvement of the servomechanisms on 37-mm. and 40-mm. guns.

In April 1942 the first joint operation of the T-10 director with the Radiation Laboratory's XT-1 radar was attempted. Results were highly encouraging from the beginning, and it was none too early to begin the long, uphill process which ultimately put these two devices ashore on the beachheads in their fighting designations as the M-9 director and the SCR-584 radar.

Additional contracts during the spring included those with the Bristol Company for a rocket director, with the McMath-Hulbert Observatory (University of Michigan) for the development of a pneumatically constrained gyroscope element, with the American Gas Association Testing Laboratory for the apparatus required to keep helium in a height finder, and with the Westinghouse Electric and Manufacturing Company for the development of a servomechanism for medium-caliber guns.

With the establishment of the Franklin Institute laboratory there was



The Bristol Co.

The Antiaircraft (rocket) Director T-18, developed to control the fire of a battery of 3-inch antiaircraft rocket projectors

RCA Photo

The Computron replaces a hundred or more tubes of the more common variety in digital computers

a mechanism available to carry out many of the instrumental developments required in the field of airborne fire control. There were still lacking, however, facilities and a program for measuring the quantitative performance of service equipment, particularly the various gunsights and computers intended for use in bomber defense. Even a casual study revealed the magnitude of the job and it became apparent that a considerable program would be required merely to find out what to do and how to do it.

Accordingly the basic outline of a project was prepared for discussion with Service representatives. This was presented at a joint Army-Navy-NDRC conference in the early summer of 1942. The outline argued the need for an effective method of testing air-gunnery equipment on the ground under conditions which simulated as fully as possible the kinematics and dynamics of air combat. There was no intention of attempting to displace air testing — however unsatisfactory it was — but rather to do for airborne computers what the Dynamic Tester had done for antiaircraft directors. The job was much more difficult, however, partly because of the necessity for including the effect of own plane's motion in yaw, pitch, and roll, but even more because of the nature of the man-machine combination found in a typical aircraft turret and computer. There the man was literally built into the system and had to be put into the Dynamic Tester too.

The matter of improving the mechanisms and procedures for air testing was also an issue but the ground tester was given first priority because it was considered the procedure which would give the quicker result in terms of influencing the choice and method of using air-gunnery equipment.

A short time earlier Fry had fortunately made a trip through parts of the Southwest, in which he learned that substantial research facilities were still available in that area. A preliminary meeting was held with the research directors of a number of geophysical laboratories located in Texas and Oklahoma for a discussion of the ground tester proposal. It was decided that the best procedure would be to negotiate a prime contract with a university in the area and enlist the efforts of the surrounding industrial laboratories under subcontracts. A strong impetus was given the program when the Physics Department of the University of Texas accepted this central responsibility.

During the remaining months of Section D-2 operation, the University of Texas and the subcontracting laboratories of the Shell Oil Company, the Humble Oil Refinery Company, Stanolind Oil and Gas Company, and the Hart Brown Geophysical Company struggled with the detailed and exasperating search for a basic method. A good solution was found, built, and put into action, but that story must await the arrival of Division 7.

The midsummer of 1942 brought a flurry of new contracts in addition to that with the University of Texas. A former contract with Tufts College had operated to carry work on various psychological and physiological problems not only at Tufts College, but also at Harvard University, Brown University, and the Ohio State University. For administrative purposes separate contracts were negotiated with each of these universities. Columbia University contracted to carry out probability studies in connection with plane-to-plane fire. The Stanolind Oil and Gas Company undertook the development of a device for determining aircraft pursuit course, needed in connection with the development of a dive-bombing sight for the Navy.

At the request of the Ordnance Department an investigation was started at the RCA Manufacturing Company laboratories on the possible use of electronic computing devices for antiaircraft directors and other computation problems. While the results from the standpoint of fire control were negative, this project did bring out valuable contributions to the general art of computation, some of which are being exploited in postwar programs, both for military and for general scientific use.

Changes in Army organization had moved antiaircraft testing to Camp Davis, North Carolina, where a large section of the Coast Artillery Board was assigned to function as the Antiaircraft Artillery Board. The change was not a pleasant one for the Section from the standpoint of accessibility and general facilities, but—*c'est la guerre.*

Significant activity was brewing on the possibility of designing a new director or modifying an existing director to take account of curvature in the path of a plane when engaged by antiaircraft artillery. The problem was under study at the Bell Telephone Laboratories, and at the Massachusetts Institute of Technology where Wiener and Bigelow had demonstrated a laboratory device which was rather eerie in its abil-

ity to predict at least certain types of arbitrary curvatures. These were merely the signs that another major study was gestating; it remained for Division 7 to carry on in this field.

In the midst of this busy summer of 1942 Caldwell departed on a mission to England, largely in connection with his fire-control radar and airborne-equipment duties. He was joined a few weeks later by Fry, who partly shared the same mission and also was assigned to investigate the status of radar countermeasures, particularly as they influenced the use of radar in fire control. An interesting outcome of the joint mission was the discovery of a penetrating analysis of the U-boat situation in the Bay of Biscay made by a member of the Operations Analysis Group of the RAF Coastal Command. This document had received very little attention yet it contained a striking demonstration of the vulnerability of the entire U-boat campaign to a small force of heavy bombers equipped with 10-cm. radar and operating from British bases. It was brought back to the United States and expedited through top OSRD channels to corresponding levels in the War Department. From there on Section D-2 lost sight of it in the stratospheric security of military operations, but long afterwards Mr. Hitler was heard to howl about exactly that subject, and maybe—just maybe—the Section helped make him howl a little earlier than he might have otherwise.

A more direct antisubmarine campaign was initiated by Section D-2 during the summer through liaison with the Navy's AsDevLant (Antisubmarine development unit, Atlantic) located at Quonset Point, Rhode Island. Philbrick took over from Captain (then Commander) A. B. Vosseller a proposal for a low-level bombsight which would continue to predict a submarine's position after it submerged and thus permit a more effective depth-charge attack. This device passed through several stages of development at the Franklin Institute, a story which must be deferred until the Division 7 narrative.

During the summer E. G. Pickels of the Rockefeller Institute joined the Section to work in the field of airborne gunnery. His arrival coincided with a revival of Service interest in the so-called own-speed computer, or vector gunsight. This device took into account the allowance an air gunner had to make for the forward motion of his own vehicle so that he could aim by allowing only for gravity drop of the bullet and for the speed of the target along its flight path. Pickels under-

took the investigation of this field and carried the activity over into the work of Division 7.

It was now well into the fall of 1942. The Bell Telephone Laboratory accepted a contract to develop a field modification kit for improving the M-7 mechanical director, applying the new knowledge gained in the development of the M-9 director. The Barber-Colman Company contracted for the development of devices to permit combined tracking and ranging, a procedure desirable for use in medium-caliber antiaircraft control. The Mark 32 torpedo director developed at the Franklin Institute had reached the stage of imminent production. With the assistance of the OSRD Transition Office a contract was negotiated with the International Business Machines Corporation for a production design and the preparation of manufacturing tools. Further exploration of gyroscopic computer elements was started through a contract at the Wilcolator Company.

The antiaircraft work of the Section was handicapped by the location of the test facility at Camp Davis, North Carolina, where it was quite difficult for the Section members to go without considerable loss in travel time. Yet the program of co-operative work between the Antiaircraft Artillery Board and the Section was important, large, and still growing. In order to support this work more effectively a contract was set up with the University of North Carolina to provide shop and transportation facilities for the Section near Camp Davis and to employ a full-time technician to assist in maintaining there the continuity of the Section program.

The work of the Princeton Laboratory at Fort Monroe began to taper off with the removal of antiaircraft activity to Camp Davis. Although radar was sweeping ahead as the outstanding means for obtaining target range, the optical range finder was by no means a dead issue. Moreover the studies carried out at Fort Monroe had resulted in the accumulation of a large bulk of new knowledge, knowledge of the sort that would have influenced range-finder design profoundly had it been known when the designs of existing instruments were executed. There was strong opinion among Army, Navy, and NDRC authorities that the advances thus made should be consolidated before the Princeton project terminated and the personnel dispersed. These discussions carried over into the work of Division 7. The final contract of Section D-2 was with

the Bausch and Lomb Company for the design of an invar bar to be used as the basic optical support of the M-2 height finder.

Section D-2 had enjoyed highly pleasant and co-operative relations with British officers and scientific personnel. The records of the fall of 1942 reveal very close working arrangements, particularly in matters concerning antiaircraft fire control and airborne devices. From the time of the Weaver-Poitras mission to England the Section had been aware of the British gyro gunsight Mark III for airborne use, and had steadily advocated American interest in it because of its advanced status of development. It is pleasant now to recall this participation in international co-operation for the common good, enlivened by a spirit of give-and-take which made for mutual understanding and respect.

The date is now December 8, 1942. There is no time to pause for breath just because the job of NDRC has outgrown its organization. Americans are fighting in the gumbo of North Africa, in the skies over Europe, in and on the waters of every ocean, and up from Guadalcanal on the road to Tokyo.

CHAPTER IV

GROUND-BASED ANTIAIRCRAFT DIRECTORS

UPON THE reorganization of NDRC in December 1942 Warren Weaver, the former Chairman of Section D-2, became Chief of the Applied Mathematics Panel. Harold L. Hazen of the Massachusetts Institute of Technology succeeded Weaver in charge of fire control as Chief of the new Division 7. Weaver continued as a member of the Division along with the other former members of Section D-2. The activities of the Division were divided between five sections numbered decimally 7.1 to 7.5. These covered the fields of ground-based antiaircraft directors, airborne fire control, servomechanisms and stabilization, optical ranging instruments and their use, and fire-control analysis. D. J. Stewart, S. H. Caldwell, E. J. Poitras, T. C. Fry, and Warren Weaver were initially Chiefs of these respective Sections. In December 1943 Fry resigned as Chief of Section 7.4 and was succeeded by P. R. Bassett of the Sperry Gyroscope Company. A sixth Section, 7.6, on naval radar fire control was organized at a later date, January 1944, with I. A. Getting of the M.I.T. Radiation Laboratory as Chief. In addition to the various Section Chiefs, I. A. Getting and A. L. Ruiz (of the General Electric Co.) served as members-at-large in Division 7. Karl Wildes of M.I.T. became Divisional Technical Aide. D. J. Stewart, as Chief of Section 7.1, was assisted by Austin Norcross and G. R. Stibitz as Technical Aides.

The first major program of Section 7.1 was that of completing tests and assessment of the T-10 Director referred to in Chapter III. This was standardized by the Army as the M-9 Director. Probably the principal contribution of the Section was in connection with "smoothing." Data received from any observing device such as the telescope or radar are imperfect. They contain errors due to the tracker which usually appear, when plotted, as a roughness superposed on the true data. Since prediction of the future position of the target depends upon the rate at which the target is moving, such roughness badly distorts the predic-

tion. It was found essential to smooth out the data by some averaging process, thus reducing the effects of the tracker's errors.

The Section spent considerable time in theoretical studies of smoothing processes. In consultation with the Bell Telephone Laboratories' engineers and with Norbert Wiener of M.I.T., they attempted to find the best practical solution. Much of the improvement of M-9 (the offspring of T-10) over M-7, the previously standard mechanical director, is due to the improved smoothing of data.

While carrying out tests on the M-7 and T-10 it became apparent that a great deal of time could be saved if certain test equipment were available. The Barber-Colman Dynamic Tester which supplied to the directors synthetic data, either in practically perfect form or with added perturbations, had been developed under Section D-2. This device was extensively used. However, since, in addition, tests were required in which actual targets are tracked it was necessary to record data on the test range and to calculate from these data the errors committed by the director. In one test thirty cameras were employed to record the position of as many dials at one-second intervals. The troubles encountered in locating and controlling such a large number of cameras delayed the tests to such an extent that it was obvious that better recording equipment ought to be developed.

To make the recording as fully automatic as possible and to avoid the frequent failures to which the photographic method seemed susceptible, the Section decided to set up a project for the construction of a "data recorder." A scheme was outlined and discussed with Colonel R. H. Kreuter of the AA Board. He agreed that the device so outlined would be a valuable adjunct to fire-control development and a contract was let to BTL for its construction. Eventually four units, each capable of recording the data from six selsyn channels, were mounted in automotive trucks to provide field testing equipment.

Meanwhile the dynamic testers had proved of great value and it became desirable to have more test courses of typical aircraft maneuvers available. Hence the Section devised and proposed a modified form of dynamic tester in which the course data were to be placed on punched teletype tape. The advantage of this method was that more courses could be produced with a practicable expenditure of time and money. The Tape Dynamic Tester was built by the Bell Telephone Laboratories

and was used in the evaluation of a number of curved-flight predictors. An auxiliary device called the relay interpolator was proposed by this Section and proved to be a great help in the production of the punched-tape courses.

The Section felt that it would probably be wise to carry on a second director program as insurance against possible failure of the T-10 Director, and also as a vehicle for certain ideas which had developed in the interim since the initiation of the T-10 program. Work was continued on the project started by Section D-2 for a second electrical director. This director, the T-15, was designed by the Bell Telephone Laboratories in collaboration with this Section. It was built and tested, and found to be, in the opinion of this Section and of the Board, somewhat superior to the M-9. However, since the latter was already in production and had shown itself to be satisfactory, the T-15 was not standardized.

In the late stages of the war, as AA became more effective, enemy planes were forced to take more evasive action and to avoid the straight-line flight for which the standard directors were designed. In consultation with the Ordnance Department and the Bell Telephone Laboratories, three curved-flight projects were initiated. One of these involved a very substantial modification of the T-15 Director. This was by far the most elaborate of the curved-flight projects.

The second project was to modify the M-9 in such a way as to compute data for curved flight.

The third project was sponsored by the Ordnance Department and provided for the construction of a plotting board, on which pens traced the present and predicted positions of the target, assuming a constant acceleration which was introduced manually. An observer watched the two pens and by adjusting the accelerations attempted to make the future-position pen predict the course of the target.

All three of the curved-flight projects were tested by means of courses on the Tape Dynamic Tester. Further tests were run at AA Board at Fort Bliss. It was concluded that the three different types of curved-course prediction seemed to be roughly comparable in giving some gain. The increase in probability of hitting under conditions which seem likely to occur in practice, however, did not make a clearly convincing case for adoption.

At the time the Section 7.1 started its work, the 40-mm. fire control was in a very unsatisfactory state. It cannot be said that a complete and satisfactory system had been developed even at the termination of the war.

The M-5 Director depended for its operation upon certain observations of the tracers, the condition for such observation being very difficult to obtain in the field particularly against high-speed targets. A project was started for the development of a simpler and more accurate 40-mm. director. The T-21 was built at the Barber-Colman Company. It proved to be simpler and more accurate than the M-5, but it also developed upon testing that the improvement in effectiveness was due largely to the addition of a range-finder spotter. This device showed the tracer as a pair of colored curves which, when the range finder was set on the target, intersected at a point in the target plane. When the range finder devised by Eastman Kodak Company was incorporated in the M-5, the performance of the director improved a great deal. While it was probable that the T-21 would have been easier to manufacture, in view of the fact that the M-5 was already in large-scale production the Army decided to modify the M-5 into M-5A2 by the addition of the range finder rather than to attempt the production of T-21.

An electrical multiplier developed for use in T-21 by the G. M. Laboratories, Inc., was later modified and incorporated in M-5A2.

To be effective in modern warfare, AA equipment ought to be able to contend with unseen targets. Existing radars are in general large, expensive, and bulky machines. While suitable for use with the heavy 90-mm. and larger guns, these radars are not adapted for use with the numerous and mobile 40-mm. and smaller guns. At the close of the war the Section had in process a makeshift arrangement for operating a number of 40-mm. guns from one radar.

Because of the highly mobile character of the 40-mm. gun itself, it was desired to mount on the gun some form of light, inexpensive, emergency fire control, which could be put into operation as soon as the gun was set up. The Weissight was proposed by the Antiaircraft Artillery Board and its development was carried forward by the Section at the Pitney-Bowes Company. The sight consists of an arrow whose direction is set by the gunner parallel to the motion of the target, and a speed adjustment on which the operator sets up an estimate of the target's

speed. It differs from the British course and speed sight called the Stiffkey-stick in that the Weissight arrow maintains a fixed direction in space as the gun turns. It was standardized as the M-7 sight.

The Section dealt continuously with the Antiaircraft Artillery Board. A project set up at the University of North Carolina under Section D-2 provided a technical representative of the Section at the Antiaircraft Artillery Board, Camp Davis, North Carolina. It also supplied shop facilities for various small jobs in connection with the work at the Board. This arrangement was continued by Section 7.1. Under this project Paul Mooney assisted with the testing of NDRC equipment and acted as liaison between the Board and the Section. Devices built included a dummy director to supply certain types of data for the testing of gun servos, and a number of slide rules, utilizing lengthy motion-picture films to serve as scales, designed to help in the computation of the triangulation problems which arise in connection with accurate location of targets.

Several of the projects set up by Section 7.1 were intended to explore fields which might lead to devices for improving antiaircraft fire control. In some cases the exploration led to the development and design of equipment, but in several instances the results were negative, as might be expected, in the sense that it did not appear worth while to carry the project through to the stage of a finished mechanism.

The first project of this nature was that set up at the University of California. From a study of the general theory of prediction, the group branched off into the design of a predictor based on geometric similarity. The mechanism was judged to be unsuited to production, and the project was closed after the final report was submitted.

It occurred to several people that the order of accuracy required of AA computers was such that a digital computer might be worth while. A Navy project at Radio Corporation of America terminated about the time that Division 7 became interested in the subject, and an NDRC contract was set up to carry on the work begun there, with the hope that an electronic digital computer might be evolved. The results, while of great interest, were not promising for the near future, and the developments in the M-9 and T-15 were such that further work on the electronic device was considered unnecessary.

A project at the Iowa State College developed in the opposite direction.

Originally intended as a project for producing a director design, the work turned toward fundamental studies, and terminated with a report on the effectiveness of various types of tracking devices.

A project was set up with the Bryant Chucking Grinder Company for the study of possible means of prediction through the use of mechanical devices, as in the Army mechanical M-7 Director, but with the added advantages accruing from the inclusion of electrical and electronic equipment. A report on this project presents a large number of radically new schemes for handling tracking data, ballistics, and computation.

Changes in military requirements and strategy rendered several of the Section's developments obsolete before they reached the production stages. A rocket director by the Bristol Company and a chart-type smoother made by the same concern were in this class. The latter was made obsolete by the abandonment of SCR-268 as a gun-laying radar. A smoothing device designed by the Bell Telephone Laboratories under the supervision of Division 7 was intended to be added to the M-7 Director. Standardization of the M-9 precluded use of the device. The Barber-Colman Company designed a lightweight tracker for use with the T-15 Director, but the project was dropped when the Army decided not to standardize the T-15.

Studies were carried out, and a number of short-base range finders were made at the Polaroid Company and at Eastman Kodak Company.

At the request of the Ordnance Department, an antitank sight for the 75-mm. gun on the M-3 motorized gun carriage was designed and built at the Barber-Colman Company. It was tested and found satisfactory, but the Army did not request further work on the sight.

At the close of the war, a gyro sight proposed and built by the Baker Manufacturing Company was tested at Fort Bliss. The principles and fundamental design of the sight appeared to be satisfactory, but vibration difficulties prevented the tests from being conclusive. The termination of hostilities put a stop to further work on this device.

CHAPTER V

AIRBORNE FIRE CONTROL

THE HISTORY of this war as a whole will show that the tide had already turned when Division 7 assumed the duties of Section D-2 on December 8, 1942. Guadalcanal, Stalingrad, North Africa, and El Alamein marked the beginning of the end. But while the enemy's power had been matched at many points, it had not been beaten down and these few victories did not give cause for relaxation in the program assigned to Division 7.

It was the job of Division 7 not only to find means to shoot down airplanes, but also to find means to "keep 'em flying" — depending on whose airplanes they were. The Division's responsibility in the field of airborne fire control was delegated largely to Section 7.2.

At the end of 1942 there were only faint glimmers of hope appearing as far as the war in the air was concerned. The RAF had gathered itself for some of the first large raids on German production centers, but the American 8th Air Force was showing only its early small efforts. A considerable concentration of American aircraft had been achieved in North Africa, but the Atlantic submarine war still made serious demands on our limited production. In the Pacific the outlook was far from bright. Severe losses had forced the Navy to use a single aircraft carrier for operations conducted in a bewildering variety of places — one of the most magnificent bluffs in all naval history. These matters were not the immediate concern of Section 7.2 but they did sharpen the emphasis on the rising tempo of air warfare and the associated fire-control problems which were the concern of the Section.

The personnel of Section 7.2 consisted initially of the men who had carried the principal responsibilities for airborne fire control within Section D-2. These were: Samuel H. Caldwell, who was appointed Chief of Section 7.2, John B. Russell of Columbia University, George A. Philbrick, and E. G. Pickels. They were almost immediately joined by A. L. Ruiz of the General Electric Company, who served as a mem-

ber of both Division 7 and Section 7.2. Early in the summer of 1943, C. G. Holschuh of the Sperry Gyroscope Company, W. A. MacNair of the Bell Telephone Laboratories, and G. E. Valley of the Radiation Laboratory accepted appointments to Section 7.2. Miss Ruth M. Peters had been assigned by the Applied Mathematics Panel to work with Philbrick in May 1943, and became a Technical Aide of Section 7.2 in September 1943. F. E. Martin was appointed to the Section in October 1943, and H. C. Wolfe in June 1944.

S. W. Fernberger of Section 7.4 undertook to guide Section 7.2 in matters pertaining to psychology and from April 1944 on he attended Section meetings regularly in this capacity. During 1945, K. L. Wildes of Division 7 devoted part of his time to the work of Section 7.2 in the field of gunnery.

The reorganization of NDRC came at a time when airborne fire-control activity was accelerating sharply. Two large and growing projects, at the Franklin Institute and at the University of Texas, were already under way, and others were in the planning stage. This situation and the growing administrative complexity of OSRD combined to cause a necessary division of responsibility between Division 7 and Section 7.2. OSRD was responding to more and more calls for vital service, and in order to discharge these increased obligations with some semblance of control there had to be a regrettable but necessary formalization of many administrative processes. In order to allow the Section to concentrate largely on the technical aspects of its program Division 7 "ran interference" on most of the red tape associated with the never-ending battle of the budget, and the various formalities set up to satisfy the legal requirements of government.

In turn, Section 7.2 provided Division 7 with the technical basis needed for its actions by scheduling its meetings to occur immediately before those of the Division, and by supplying technical summaries and recommendations. These came either directly through the Section Chief, or through written contributions to the Division's bimonthly summary report, and reports from contractors. Individual members of Section 7.2, including some of those who served without compensation, shared the detailed duties associated with the planning and execution of specific parts of the Section program. They in turn reported problems and progress back to the Section as a whole for such action as

was judged necessary to serve best the ever-changing needs of the air war.

In its relations with Service groups the experience of Section 7.2 was varied. That experience had a profound influence on the kind of work the Section did and is an important part of this history — if the history is to have any meaning for future workers. Hence, for whatever value there may be in it, a few statements of a general nature will be made; these generalizations will obviously be of the type known as dangerous.

By and large, the Section enjoyed most co-operative association with Navy groups concerned with airborne fire control. Sincere efforts were made to vitalize the work of the Section through prompt disclosure of battle data, and programs were planned on a man-to-man basis which exploited the power of true co-operative action. There was no aspect of perfection in all this, for indeed some officers rendered only lip service to the idea of Navy-NDRC teamwork, but on the whole the Navy used everything that Section 7.2 had to offer and made the Section feel that it had a share in fighting the war.

The principal and natural point of contact for Section 7.2 with the Army Air Forces organization was at the Armament Laboratory of the Air Technical Service Command at Wright Field. It will do no good to conceal the fact that the Section had to undertake a long, uphill campaign to win the privilege of helping to understand and solve some of the technical problems of the Armament Laboratory — a campaign which succeeded only in spots and only too often led to frustration and a sense of intrusion. Among individuals there were many exceptions, of course, and the NDRC liaison officer at Wright Field distinguished himself by his perseverance and patience in trying to foster better exploitation of NDRC efforts. But the fact remains that Section 7.2 never succeeded in reaching a plane of partnership at Wright Field which even approached that reached with equivalent Navy groups.

The Army Air Force experience was not all discouraging. Real teamwork was achieved with the Research Division of the Central Instructors' School (Flexible Gunnery) located first at Fort Myers, Florida, and later at Laredo, Texas. This organization turned frequently to Section 7.2 and other NDRC groups for help in its problems and undertook many activities under joint sponsorship. Beginning in January 1944 a civilian scientific member of that unit was detailed to attend Section

meetings. Later, when the testing programs of the Section at the Franklin Institute and the University of Texas reached great magnitude, groups of enlisted personnel were detailed to act as laboratory subjects and to render assistance to the Section in these programs.

To a somewhat lesser extent the Section was able to contribute to the work of the Army Air Forces Proving Ground Command at Eglin Field, Florida. There the problem was partly one of laying the scientific foundations of a sound program of measurement in the air, but it was even more one of working with current and urgent studies which had to be made regardless of the degree of compromise involved. The latter type of problem would have taxed the facilities of Section 7.2 beyond their capacity and hence no more than advisory action and some construction of special equipment could be attempted.

The technical program of Section 7.2 started with the informal studies carried on by Section D-2 and the two formal projects set up at the Franklin Institute and at the University of Texas. When Division 7 was organized a new proposal had already been prepared for a project at the General Electric Company and this was acted upon immediately by Division 7.

This project was concerned with the development of a new computer for a central fire-control system of the general type employed in the B-29 airplane. A year earlier the Army had contracted with the General Electric Company for a computer to be used as part of a complete radar gunnery system on very heavy bombardment planes. By the beginning of 1943 the computer so developed had been put into production in order to be ready for B-29 production. In the course of its development many ideas had arisen which could not be investigated in time to meet the freezing date of the production-type computer. Necessarily, then, the computer made for the B-29 was not a highly developed device. In agreement with the Army Air Forces, Section 7.2 undertook by its contract with the General Electric Company to back up the B-29 production program by continuing the computer development until it represented a degree of improvement which would warrant a production change.

This was a fine idea but the program fell far short of realizing it. Initially it was delayed because the contractor did not have sufficient manpower to take care of the distractions of the production program.

Later, it was delayed by modifications required in the production equipment. Still later it was submerged under the priority of the Navy Mark 56 antiaircraft development sponsored by Section 7.6. So the whole program seemed to move from one plateau to another and eventually resulted in a new computer, completed but not tested, at the end of the war. Some profit was derived from the fact that certain details of the new development were applicable to production equipment and were so used. A larger profit (to the over-all war effort) came from the fact that some manufacturing methods developed under this contract were applicable in accelerating the progress of the Mark 56 antiaircraft system mentioned above. The computer finally built has been turned over to the Armament Laboratory of Wright Field for evaluation and further action.

Another active program taken over from Section D-2 was the Torpedo Director Mark 32 which had been developed for the Navy at the Franklin Institute. By the end of 1942 the Navy had decided to put this device into production. Section 7.2, with the co-operation of the Transition Office of OSRD, undertook the trial production of twenty-five units at the International Business Machines Corporation. This program was successfully completed although the Navy finally placed its production order with another manufacturer.

Philbrick had engaged in discussions with Captain (then Commander) A. B. Vosseller at the Antisubmarine Development Unit (Quonset Point Naval Air Station, R.I.) regarding the development of a hand-held sight for dropping depth charges in low-altitude attacks on submarines. Captain Vosseller's idea was to incorporate a mechanism which would predict the underwater position of the submarine from an estimate of its speed and course before submerging and a line of sight taken on the swirl which marked the spot of submergence. At the Franklin Institute this idea was pursued through several models of the VASS (Vosseller Antisubmarine Sight). After model IV of the device had been tested with good results it was subsequently designated by the Navy as Bombsight Mark 20 for production purposes. At the Navy's request, Section 7.2 undertook a crash program to produce at the Franklin Institute sixty of these units for squadron test and training. But this development was another casualty of tactical change and by the time it was fully ready there was no great need for it. It was kept

in reserve, however, and some improvements were completed, notably the addition of an automatic altitude input fed by servomechanism from a radio altimeter. It was also modified at Navy request for use in lighter-than-air craft and the model made for this purpose was designated Bombsight Mark 24.

Section 7.2 brought further support to the antisubmarine campaign by an extensive theoretical and experimental investigation of the so-called angular-rate method for low-altitude bombing — a method discovered by the British, as acknowledged in the code name BARB applied by Section 7.2 to the further experimental developments of the British Angular Rate Bombsight. The theoretical studies were contained in a series of documents entitled *Notes on Low-Altitude Bombing*. Experimentally, five different versions of BARB were studied, four of them at the Franklin Institute, and BARB III at the McMath-Hulbert Observatory, University of Michigan. The latter version was started to exploit the possibility of using certain pneumatic controls developed by Section 7.3. Within a very short time an experimental BARB III was available for flight testing and gave encouraging performance from the outset. For administrative reasons this work was continued under the sponsorship of Section 7.3. The story of the ultimate development of BARB III into the highly successful Bombsight Mark 23 is a part of the history of that Section. As soon as the potential worthiness of BARB III became evident, Section 7.2 discontinued work on other lines of attack in this field.

The early weeks of 1943 brought a development of first magnitude at the University of Texas project. In the planning of the testing machine for aerial-gunnery systems much time and effort had been spent on the design of the target to be used. Many ingenious schemes were proposed and studied but all of them involved to some considerable degree the problem of compensating for parallax errors caused by the limited size of the equipment which could be used and by the fact that most of the equipment could not be located at the origin of co-ordinates. These difficulties were swept aside almost overnight by the invention of the "Texas target." This device consisted of a very large version of the reflecting sight, used backwards so that instead of projecting cross hairs to infinity, the image of a target was projected to infinity. That is, the light reaching the test position from the target was parallel light.

This key step won relatively complete freedom in design and although there were many difficult problems still to be solved, the road ahead had no washouts — it was merely bumpy. With new inspiration the group at the University of Texas forged rapidly ahead on their tough assignment, and within another year they not only had one machine operating but had made substantial progress in building two more machines to be set up in Army and Navy test establishments.

As a sort of relaxation from their main efforts to devise equipment for testing sights and sighting systems, the Texas group made pertinent contributions to the design of lead-computing sights. One interesting example was their development of a method for avoiding the time lag in getting on target due to the transient behavior of the sight. Instead of a reticle, they used a constellation of light spots arrayed on logarithmic spirals. When these spirals were rotated at a speed which was a function of the reciprocal of target range, the spots of light appeared to drift inward toward a central point at rates which decreased exponentially as they approached the center. In using the sight it was merely necessary for the gunner to track his target at whatever rate made the target appear to have no motion relative to the nearest spots, and in this condition he was ready to fire.

This sight was promptly dubbed "Spots before the eyes." After an experimental verification of its theory (with the experimental evidence in some doubt because it seemed impossible to find a Texan who needed a sight!), the sight was turned over to Section 7.6 for firing trials on Navy antiaircraft problems. Section 7.2 saw no possibility of completing a design of this type for use in air gunnery without seriously disrupting the main effort of the contractor on the testing machine.

During 1942 and early 1943 Army Air Force experience, particularly in the Mediterranean theater, had led to a development of the so-called position-firing method in bomber defensive gunnery. This system assumed that the target would always be a fighter airplane which, because of the fixed direction of its guns, had to fly a curve of pursuit, or nearly so, in attacking a bomber. Under these assumptions, and for given bomber speed, the gunner was taught to position his line of sight between the target and the tail of his own plane, with a lead angle which was expressed simply as a function of the angle between his own flight axis and his line of sight to the target.

The lead angles used in position firing took into account principally the deflection needed to overcome the component of bullet velocity due to the motion of the bomber itself. Actually, since a fighter flying a pursuit course appeared to be coming almost head on, the lead was negative. It came as a rude surprise to many gunners who had been exposed to training which included skeet shooting that they had to "trail the b . . . s"[1] instead of leading them.

Position firing was never a completely satisfactory procedure, but it brought about a vast improvement in defensive shooting because it at least contained the right direction of lead and thus allowed a more favorable set of probabilities to operate. It also brought about a small storm of activity in the development and assessment of own-speed or vector sights. In theory this type of sight was supposed to mechanize for the gunner the computation of the lead angle required to compensate for the speed of his own plane, leaving the gunner with the task of placing his sight reticle on target (for a pursuit course) and opening fire. The idea was deceptively simple. In practice the process of deriving a solution by this method was never entirely satisfactory by the time all the errors in the assumptions were corrected. Moreover, a system of defensive gunnery based on these sights was highly vulnerable to changes in enemy tactics.

One of the more thorough studies of this branch of air gunnery was carried out by Section 7.2 through a contract with the Jam Handy Organization. This contractor was especially well qualified for the work from experience in producing gunnery trainers and training films for the Army and the Navy. The program called for a vector computer which could be used either to provide the reticle deflection in a gunsight, or to provide a photographic indication of the own-speed component of lead which could be used in the assessment of other gunsights.

The over-all program was considerably delayed, partly because of higher-priority calls on the facilities of the contractor and also because the main program was sidetracked in order to carry out the development of a vector sight needed by the Navy for more immediate use. The outcome of that program was the Gunsight Mark 25. The original computer-gunsight-camera combination was finally completed and

[1] birds.

turned over to the Section's assessment group at the Patuxent River Naval Air Station. But an important contribution of the Jam Handy Organization was its final report — a document which is important for its summary of the theory and modes of mechanization throughout substantially all of the vector-sight field.

The spring of 1943 also saw considerable progress in the extended series of studies at the Franklin Institute on the nature of tracking and gun errors in aircraft gun turrets. The gunner's job of tracking his target with a lead-computing sight is complicated by two facts. He operates turret controls instead of controlling his sight directly. The sighting reticle has a motion related to that of the turret by a differential equation. It was possible to idealize the target courses and to represent both the sight and turret characteristics by quite accurate mathematical expressions which could be varied by manipulating the values of numerical coefficients. This simplified the organization and mechanization of the program tremendously, but no way was found to simplify the man — he had to be included without modification.

It was evident that while the design and construction of the equipment needed to represent the target, the sight, and the turret control were straightforward jobs of engineering, the subsequent use of the equipment by experimental subjects and the interpretation of the results were matters which demanded careful supervision of physiological and psychological conditions, and the sound application of statistical techniques. These needs were met by Malcolm Preston and Frank Irwin of the University of Pennsylvania — both of them psychologists and both trained in the application of statistical methods. They were retained by the Franklin Institute to supervise the execution of the program laid out by Section 7.2 during the next two years, and to prepare the series of reports issued on these experiments.

The equipment used in these studies underwent radical evolution during the course of the work. At first a mechanical simulation system was used, designed to permit modification of the sight characteristics by changing gears. The subject observed a fixed target spot and a reticle which moved away from it under the action of a cam system. His job was to keep the reticle on the target by manipulating a turret-control handle. This controlled the reticle through a mechanism simulating the dynamics of a turret with a lead-computing sight. Since the gun

position, reticle position, and target position were all available within the mechanism, it was possible to record both tracking errors and gun errors on charts which could be analyzed statistically to determine the errors committed.

After some experience with the first model of the tracking equipment a number of changes were proposed to secure increased flexibility, and more accurate and more reliable operation. A second mechanical unit was partially designed but work on it was halted in favor of an electrical design which proved to be highly successful.

The third stage in the development of this equipment involved the use of an actual turret instead of a system which simulated turret dynamics. From the experiments performed up to that time the unqualified conclusion had been drawn that aided tracking was superior to velocity tracking for turrets containing lead-computing sights of practical design. Since the application of aided tracking to an inhabited turret involved a possible severe physical acceleration applied to the gunner, it was necessary to use a full-size turret in order to be sure the earlier finds were sound.

A new target-and-reticle-presentation system was developed, using a cathode-ray oscilloscope tube mounted on the gun cradle of the turret. Fortunately the recording equipment developed for the electrical tracking machine was applicable in the turret system. The results of the experiments carried out not only confirmed the earlier conclusion, but gave specific information regarding the optimum values of the parameters in both sight and turret.

This sort of information would have been of great value if the war had lasted longer but it came too late to exert an influence on the actual equipment used in the air. The general situation of airborne gunnery in the middle of 1943 was a muddle, and largely because no one, including this Section, had a solid foundation of fact upon which to base improvement. The Section had vector-sight developments under way at the Franklin Institute and at the Jam Handy Organization, and a central-station computer under development at the General Electric Company. And in other establishments dozens of similar developments were in process. Everyone had to participate in this shotgun type of program, hoping that at least one line of attack would be successful, because no one was sure which direction should be followed. At one end of the

scale there were the ambitious, all-radar programs for heavy bombers; at the other end of the scale was the raw fact that gunners in combat could lead their targets in the wrong direction.

In this general melee Section 7.2 decided to resist any expansion of its sight-development program and to concentrate its further effort on the job of learning how to measure the quality of gunnery. The Franklin Institute and the University of Texas programs were already under way and increased effort was urged on both these contractors. At the same time discussions were opened with the Navy which led by the end of the year to the establishment of an air-measurement program at the Patuxent River Naval Air Station.

This policy of Section 7.2 had no glamour about it and no one could expect from it telegrams of congratulation from the battle zones. It was a policy of slugging to get facts, by clever ways if possible and by the hard way if necessary. The faith of the Section in the fundamental soundness of its action was rewarded only in a few exciting months as the war neared its end. Then the technology of measurement and analysis reached the point where categorical answers could be given to the questions "How good?" and "Why?"

During the remainder of 1943, following the completion of the Mark 32 Torpedo Director development, the Section continued some further work on torpedo directors, but the general disinterest of the fleet in such devices brought about a reduction of the facilities allotted to such work and a strong increase of sales resistance within the Section. Nevertheless, important advances were made at the laboratory stage on the grounds that a need might arise and it was better to be prepared to satisfy it. A great deal of work was done at the Franklin Institute on the Navy's Torpedo Director Mark 30. The principle of target-course stabilization was the outstanding new feature of this director. A built-in computing mechanism was provided later to replace the slide-rule chart which had been used with the original Mark 30 torpedo director to take into account variations in the release conditions.

These developments were carried out primarily for the Navy, which enjoyed, by agreement at high level, a monopoly on the use of torpedoes. Wright Field continued its interest in the subject, however, and Section 7.2 co-operated by making the modifications necessary to supply

Army Air Force versions of every important torpedo-director development. This action was taken because the Army had a lot more airplanes than the Navy, and because if the policy ever changed and the Army began to tote torpedoes, there would be a sudden and violent demand for Army torpedo directors — if for no other reason than to train their pilots in the use of whatever the Army called its "seaman's eye."

A basic difficulty of all torpedo directors was the necessity for using the torpedo run (future range) instead of the present range of the target as an input variable. In terms of torpedo run an explicit and simple computation of the aiming angle was possible, while the solution in terms of present range was transcendental in character and difficult to mechanize. The problem of developing a present-range type of director was kept active at very low level for many months. That policy paid off when a new linkage system was devised — a sort of implicit linkage — to accept the present-range variable. It was mechanized successfully and finally adapted to take radar range by means of a servomechanism.

One further torpedo-director development was undertaken in co-operation with the Applied Mathematics Panel. This was to produce a director which would make some provision for evasive action taken by the target. Obviously, a complete solution of this problem would have required some sort of output from a crystal ball. However, the Applied Mathematics Panel was able to provide data on the release conditions required to maximize the probability of hitting a maneuvering target for a number of reasonably practical cases. These data were studied at the Franklin Institute to determine a mechanism design which would compute the release condition. At war's end it was possible to report what appeared to be a feasible method, for which some vital components had been checked experimentally.

By the end of 1943 Section 7.2 was engaged in a variety of bombing projects, of which the Bombsights Mark 20 and Mark 23 have already been discussed. In co-operation with the Air Bombing Training Unit of the U.S. Naval Air Station, Banana River, Florida, the Franklin Institute laboratory developed: (1) a computer for use in presetting the synchronization controls of a Norden bombsight so as to reduce the length of the straight-line bombing run required; and (2) a bombing

"bug" to replace the standard target used with bombing trainers, this device incorporating controls for simulating evasive action taken by the enemy.

The Section also initiated a study at the Eastman Kodak Company of the problem of assessing the merits of bombsights and bombing systems, along lines analogous to the gunnery-assessment project at the University of Texas. This project was limited to a study and outline design phase. When that part of the work had been done, the Section reviewed the results and decided that there was not enough hope of obtaining war-useful data in time to be significant. Accordingly these preliminary studies were turned over to the Army and Navy for consideration in their postwar programs.

An important development of 1943 was the formation of the so-called Stratton Committees. These were committees formed under the chairmanship of J. A. Stratton, of the Office of the Secretary of War, to survey developments and recommend Army policy in the use of radar for aerial gunnery and blind bombing. The Chief of Section 7.2 served on both committees. Pending the receipt of recommendations from the Committee on Radar Aids to Bombing, the Army had proceeded with the application of the Eagle radar system to large bombers. Late in 1943 Wright Field requested Division 7 to make a survey of the computers available for use with this radar and to recommend the computer to be used. A group consisting of Hazen, Caldwell, and Ruiz carried out the survey and reported their findings. Shortly thereafter the Stratton Committee decided that a more universal type of radar should be recommended, and requested Division 7 to extend its survey of the computer field and recommend the computer system to be used with such radar. The same Division 7 group worked on this assignment and reported its recommendation of the G.P.I. (Ground Position Indicator) system developed at the Radiation Laboratory.

In November 1943 Section 7.2 negotiated a contract with Columbia University in order to provide additional development facilities, particularly in the field of electronics. The work at Columbia was carried out independently but was co-ordinated by the Section with the larger and more diverse activity at the Franklin Institute. One of the first projects at Columbia, undertaken in co-operation with Division 5, was to develop an electronic simulator to represent the dynamics of a

guided-missile situation. Initially this was done for the Razon bomb, while the Franklin Institute worked on the bombsight itself. The Razon simulation, although initiated in order to get design data, was so successful and realistic that it was taken over as a whole by Division 5 to satisfy the requirement for a Razon trainer. The work on simulation of guided missiles at Columbia continued for more than a year, until it was transferred to Division 5 auspices, with a uniformly high degree of success in analyzing the difficult control problems encountered in the design and use of guided missiles.

As was remarked above, the actual bombsight instrumentation for operational control of guided missiles was started at the Franklin Institute. An ingeniously simple attachment to the standard Norden bombsight was found to make it effective in the control of Razon. This attachment — known as Crab — was put into production by the Army. More elaborate attachments — such as the one known as Carp — were needed to control more maneuverable types of missiles. Development in this field again proceeded with Columbia University simulating the action electronically and the Franklin Institute using the data thus obtained to design equipment. The responsibilities of Section 7.2 in the general field of guided missiles were substantially fulfilled by the end of 1944 and shortly thereafter all such project details were turned over to Division 5. The final project in this category was the modification of the standard A-6 bombing trainer so that it could be used for bombardier training on Azon and Razon bombs. This work was done also at the Franklin Institute.

Mention has been made of the Section's program of air-gunnery measurement and assessment at the Patuxent River Naval Air Station. This project was requested in the fall of 1943, following discussions with officers of the Bureau of Aeronautics and the Bureau of Ordnance. The request itself was an endorsement of the Section's campaign for improved quantitative understanding of the gunnery problem, and gave the Section broad latitude in seeking its objectives, using Navy personnel and facilities.

The Section was fortunate in finding an experimental group at Northwestern Technological Institute which was just terminating work on another contract. This group, under J. F. Calvert, started work on the Patuxent assessment program in March of 1944 and had reached

the stage of building computation machinery and designing measurement equipment for use in airplanes by the following summer.

It was becoming increasingly difficult to find personnel and facilities at the beginning of 1944. Manpower shortages were especially acute, and were complicated by the constant need to hold scientific workers from Selective Service call. At the same time the increased preoccupation of the armed forces with purely military problems was creating increased need for civilian scientific service. The Section was faced with an almost continuous problem of regrouping its facilities in order to adjust to sudden demands and priority shifts.

In the latter part of 1943 Section 7.2 had been asked by the Navy to assist in the development of a sight for use in rocket attacks on submarines. By the spring of 1944 substantial progress had been made on the rocket sight known as RASP. Thereafter the Section undertook a greatly increased program of rocket-sight development at the Franklin Institute. Much of this was at Navy request and with the co-operation of Division 3. Both agencies provided the use of facilities at the Naval Ordnance Test Station, Inyokern, California. The principal instrumental outcomes of the development were the rocket sight known as GRASP, based on the use of the Gyro Gunsight Mark 18 as the indicating element, and another known as PARS, this being an instrument operating at low energy level and having all computing elements contained within an altimeter case.

A still more important outcome, however, was the concept of a sighting system for the pilot of a fighter plane. The fighter airplane was rapidly becoming a most potent weapon of attack which could carry guns, bombs, rockets, or torpedoes. Each of these missiles involved a different computation problem. Under the stress of war the versatility of the fighter plane had flourished, but instrumental aids to the pilot were virtually nonexistent. The problem of rendering help in this situation was especially difficult because of the stringent requirement that the sight had to *help* the pilot, and not give him so much of a task of sight manipulation that he couldn't fly his plane. The Section initiated its program of development in this area early in 1944 and was hence off to a good start when the Navy requested such work in the summer of 1944. This became known as project PUSS—the Pilot's Universal Sighting System. It will be discussed further at a later point.

The general confusion in the field of aerial gunnery has been remarked upon. This came to a climax in early 1944 when many items under development reached or neared completion and the Army Air Forces faced the task of deciding which if any of many devices should be used. Section 7.2 was asked by the office of the Assistant Chief of Staff for Operations, Commitments, and Requirements in the AAF to survey the situation and recommend procedure. Even before this task was accomplished the Army Air Forces set up a committee representing various interested agencies such as Wright Field, Eglin Field, Training Command, etc., and requested NDRC to supply a Chairman for the committee. The request was assigned to Section 7.2, and J. B. Russell, who was also doing much of the gunsight survey, was appointed Chairman of the Airborne Fire Control Committee.

At first this was purely an Army affair, but because of its status as an NDRC project the Navy assigned a large number of liaison officers. (On at least one occasion there were more liaison officers present at a meeting than there were committee members.) It was soon changed to a joint Army-Navy project, retaining the NDRC chairmanship, and at the request of Section 7.2 the Applied Mathematics Panel designated Saunders MacLane to serve as Vice-Chairman.

The Airborne Fire Control Committee started out with two principal duties. First, to co-ordinate developments in gunnery devices. Second, to set up effective test and evaluation procedures. This was an interesting committee in that it was given these much-needed objectives but it was given no mechanism to carry out experimental investigations and no authority to execute its recommendations. It got around those difficulties very neatly by attracting the participation of men who had the necessary facilities and the necessary authority to get action. Both the Applied Mathematics Panel and Section 7.2 had learned the technical background of aerial gunnery and this combination was a potent one in dealing with the questions brought up for committee discussion. The discussions were cast at such a useful technical level that more and more of the responsible officers in both Army and Navy found it profitable to use a day once a month to attend the committee meeting. This led inevitably to the expansion of interest throughout the general field of airborne fire control. The committee expanded its activity by soliciting prepared papers on various fire-control topics in order to

cultivate further a widespread understanding. By the end of the war this committee had become a most important forum in the airborne fire-control field. The main committee, acting in executive session, spoke with authority, an authority derived from its quality rather than from a standing order. Its only failure was its inability to find a mechanism for continuing the role it played, on a postwar basis.

The summer of 1944 found every facility of the Section loaded with work. As the Allied armies pressed their successful invasion of the western European continent, research attention swerved sharply toward the problems of the war with Japan. The fundamental programs were continued with all possible pressure. The Texas tester was in use on its first assessment problems and another machine was being erected at the Patuxent River Naval Air Station. At the latter location also a group from the Northwestern University project were designing the elaborate synchronized-camera system required to measure gunnery performance in the air, including radio links to tie together the cameras in the fighter and in the bomber. Meanwhile, back in Evanston another part of this group was building up the computation production line which was needed to unravel the tangled skein of co-ordinates in which the pictures were to be photographed and then to present the final results in understandable form.

It is most pleasant to recall the all-out co-operation of the Applied Mathematics Panel in these efforts. Large assignments of manpower were made from applied mathematics groups at Columbia University and at Northwestern University. But more important was the fact that many of these men chose to live with the problem of the Section and thus were able not only to cope with those problems but occasionally to anticipate them. The sense of partnership between AMP and Section 7.2 simply ignored organizational boundary lines, figuratively and literally. This was demonstrated once during a visit by the Chief of AMP and the Chief of Section 7.2 to the Commanding General of the AAF Proving Ground Command at Eglin Field, when Weaver said, "Either Caldwell or I can and will commit either AMP or Section 7.2 to whatever action either of us feels is necessary, and the other will back him up."

Although rocket-sight development proceeded at a feverish pace during the summer of 1944, a more significant matter was the crystallization of the PUSS program. This was set up at the Franklin Institute

and the initial assignment was the development of the rocketry component of PUSS. Actually, this did not limit the program a great deal because whatever sight head was used would be common to the entire system, and the basic data-gathering instruments of the rocketry computer would serve also in gunnery and bombing. At this time also the Navy entered its formal request for the PUSS system.

The PUSS program was an ambitious one and could not be hurried without falling far short of its goal. Severe limitations on space and on the complexity of controls made it necessary to explore many possibilities for each element of the system. The combination of elements into a system also offered a variety of possibilities. It was anybody's guess as to when the war with Japan would end, and the PUSS system offered so much potential advantage if it got into action that the Section gave a full green light to the undertaking.

During the autumn of 1944 the load on the Texas tester climbed to a new high, especially because of the need for studying difficulties in the gunnery-control system of the B-29 airplane. The Fire Control Committee rendered important service in helping to decide the priorities which should be followed in the testing program.

The campaign in Europe swept on and hope was high. There was much discussion of how the postwar research of the air force should be conducted. At the request of the Army Air Forces, Russell left for a mission to England, particularly on gunnery problems, and the Section closed ranks to cover his normal duties. As a tailpiece to that hectic period, news was received about the development of two vector gunsights in the South Pacific!

Then occurred the unfortunate episode in which the divisions of OSRD were asked to prepare plans for demobilization, using "for planning purposes only" an assumed date of November 15, 1944, for the fall of Germany. It would be untrue to say that this false alarm wrecked any part of the Section's program, and certainly the Battle of the Bulge corrected any gross error that might have been made, but there was a distinct letdown from which some parts of the program never fully recovered. As it turned out, it was effective in decelerating a number of projects so that they could be closed out efficiently when the real end came, but the process was not selective in that it also handicapped some work which needed every ounce of energy available.

Having forgotten demobilization, the Section entered the final months

of the war with its decks cleared for action. The earlier pages of this chronicle have told the story of some parts of the program in full; these will not be discussed further.

Principal interest now centered on the PUSS program at the Franklin Institute, supplemented by a new program at Columbia University to simulate the dynamics of a fighter plane, and on the two assessment programs at the University of Texas and Northwestern University.

As the PUSS program advanced to the stage of decision on important elements, the Franklin Institute subcontracted for additional facilities to work out details, and the Section added a contract at the Bristol Company to work on some of the problems of devising a sight head. It was found necessary to keep two types of systems under development. One of these was based on electromechanical methods of computation which were well understood at the Franklin Institute. The other made use of pneumatic computing elements borrowed largely from the work of Section 7.3. Both systems had their unique advantages, and under war conditions the only way to reach the right decision was to continue both until one or the other proved its superiority. Actually neither system was completed during the war, but the project was of such fundamental importance that the Navy elected to continue it in its postwar research program.

The University of Texas project moved right up into the battle lines during the closing months of the war when E. A. Hewitt of the Operations Analysis Section of the 20th Air Force discovered the unique power of the Texas machine in simulating battle situations and in finding ways and means to overcome deficiencies in the equipment being used in battle. Progress was rapid and sometimes spectacular. Section 7.2 almost became a spectator at its own show at times, for the needs of the B-29's then flying over Japan took priority. But experiments leading to the development of still better equipment were worked in, and the construction of similar machines at Patuxent River and at Wright Field was pushed. The Navy construction was completed before the termination of this contract. The Army then took over the contract to complete the construction at Wright Field and to improve the general testing process based on such equipment.

The work started at Texas is far from being finished. It demonstrated triumphantly that sound principles of measurement were essential to

the development of aerial gunnery. But its success in 1945 did nothing more than to emphasize the pitifully small state of our knowledge and to point out one of the directions in which postwar research must continue — until guns become obsolete.

Northwestern University had reached the point of operating a complete air-measurement system and a complete ground-computation system, and was well on the way to achieving numerous refinements and extensions to the system by the time the war ended. Early in 1945, Section 7.2 had proposed to the Navy that a naval officer take over the chairmanship of the steering committee for this project. This symbolized the fact that by the assignment of personnel for indoctrination and operation the project had become, in accordance with plan, more Navy than NDRC. It was a timely step, for it permitted the Section to withdraw gradually from the active management of the project, and permitted the Navy, immediately after the surrender of Japan, to take over under its own contract what had already become a Navy project.

There is little more to tell. Section 7.2 contracts were all closed out or transferred to the Army or the Navy without loss of any essential value. The supplementary work on the PUSS system at Columbia University and at the Bristol Company was transferred to the Navy contract at the Franklin Institute. There were a few anxious moments when it appeared that the Army Air Forces budget would not take care of continuing the work at Texas, but the money for it was eventually found.

Section 7.2 did not conduct a perfect war program. But out of its successes and its failures one lesson was learned. Considering the air-gunnery case as merely an example of a general truth, it was demonstrated that progress was slow and confused when little was known or could be measured. American defensive-shooting superiority in the war was derived not from the quality of our fire-control equipment but from the use of prodigious quantities of ammunition. It took us most of the war to learn how to get the real facts, and the lesson is that the hard road is the right path in this situation. There is never enough time available to try the short cuts.

CHAPTER VI

AUTOMATIC CONTROLS

THE HISTORY of the Fire Control Division in the field of automatic controls is closely coupled to the battle history of the war. With the turning of the Axis tide in the winter of 1942–43 at Stalingrad, North Africa, and Guadalcanal came added emphasis by Division 7 upon the research and development of weapons to contribute to offensive war. Thus Section 7.3, charged with servomechanisms, gyroscopic stabilization, data transmission, and analogous problems after the reorganization of NDRC, was in the months following to place particular importance upon the development of airborne instruments such as bombsights, aerial-camera stabilizers, torpedo controls, etc. These devices resulted primarily from certain pneumatic instrumentation techniques developed by the Section.

A recitation of accomplishments resulting from the exigencies of the war in the form of formal projects completed, and of Army or Navy adoption of newly created instruments of war, gives in substantial measure the history of the Section. However, a history based merely on neatly classified accomplishments is necessarily partial, since the Section made substantial contributions of a miscellaneous nature, and which did not involve direct contracts. These generally took the form of consultation with industrial or Service groups. Sometimes this took place upon direct invitation. In other cases some ancillary business brought problems to the attention of the Section and enabled them to make suggestions on technical matters.

Before embarking upon a recitation of the formal projects initiated by the Section, attention will be devoted first to the Section personnel and the role of the Washington Office of Division 7.

With the reorganization of NDRC on December 9, 1942, Edward J. Poitras became a Member of Division 7 and the Chief of Section 7.3. The members of the new Section were John F. Taplin, a Consulting Engineer, James D. Tear of the Ford Instrument Company, and George H. Pettibone of the General Electric Company.

The duties of Poitras as Technical Aide to Warren Weaver had involved the maintaining of a Section D-2 Washington Office, with the aid of Merriam H. Trytten and a staff of two clerk-stenographers. The reorganization extended the existing role of this office to a dual one and it became both the Washington Office of the Division and the headquarters of Section 7.3.

In February 1943, Trytten transferred from OSRD to serve in another Government agency in the field of scientific personnel. (He subsequently became the Director of the Office of Scientific Personnel of the National Research Council.) Trytten was succeeded by Lawson M. McKenzie who transferred from the Navy Department. There were no other changes of technical personnel during the life of Section 7.3.

The duties of the Washington Office were varied. Because of the proximity to the P Street headquarters of OSRD and the Navy and War Departments, it became a filter center for Divisional communications. Daily personal contacts were made relating to fire-control matters between the office staff and officers of the Army and Navy and other Government officials. A similar close technical liaison was maintained with the resident British technical representatives. Considerable exchange of information on fire-control matters was had with the many foreign technical visitors. Complete files of the Division's activities were maintained and various fiscal and administrative functions were administered by the staff. The Technical Aides in charge of the office represented the Division in its role as the advisor to the Commissioner of Patents. Hundreds of patent applications in the fire-control field were examined to determine which should be maintained secret, with their public issuance delayed. The administrative duties were the usual devolving upon a staff administering a research program in time of war: Selective Service, priorities, review and distribution of both foreign and domestic classified reports, contract voucher review and approval, property-control recommendation, surveys, and other various and sundry activities.

The method of operation of the Section was similar to that of some of the other sections and divisions of NDRC. When the need for a particular instrument of war was felt by the Army or Navy, or when a member of the Section presented a suggestion for a new weapon to the group, considerable detailed study was given the matter by

the Section and often by the Division. Then a laboratory was approached and requested to undertake the necessary development work. It was customary to discuss in detail with the prospective contractor both the need for the instrument and its mode of operation. It was considered important that the contractor be convinced of the need for the instrument, and also be sure the approach was a sound one. Once a contract had been undertaken by a scientific group, the Section members continued close contact with the project so that they could contribute along with the contractor's personnel in solving the day-to-day problems as they arose. The Section was responsible for maintaining proper liaison with the Army and Navy, by having issued suitable progress reports, and arranging visits by Service liaison officers when appropriate. In this way the Services could correlate each of these developments with their own development program.

Often the early model of an instrument, sometimes a "breadboard" model, would be given field tests by the Services; usually the contractor would have project engineers present to conduct, or help conduct, the tests. The indicated modifications would then be made as promptly as possible and a prototype would be submitted for final tests and Service approval and standardization. Upon standardization all drawings and other data available would be turned over to the Services for use by their production contractor; very often the production was undertaken by the company which had done the development work.

Occasionally it was necessary to convince the Army or Navy of the value of a new approach to an old problem. A typical activity of the Section along this latter line was the selling of aided tracking to the Services. Prior to the War there were two well-known means of tracking. One involved velocity tracking, wherein a gun or instrument would automatically move at a rate determined by the position of the handgrip or handwheel. The other was a method of tracking wherein a particular motion of the handgrip would result in a similar motion of the device being tracked. Both methods had their advantages and shortcomings.

A combination of the two methods (called aided tracking) results in a motion in accordance with the sum of the two effects; i.e., the instrument would move through an angle proportional to the motion of the

handgrip, and also continue to move at a velocity proportional to the position of the handgrip. Accordingly, the Section designed and built a demonstration set which would respond to any of the three types of tracking; position, velocity, and aided tracking were available at the turn of a selector knob. This unit was portable and could be taken before various groups and demonstrated. Thus, by tracking automobiles, and such, out of a window, one could perform qualitative experiments on tracking methods. This selling job involved many months, and the 100-pound demonstration unit had to be carried many miles before the advantages of aided tracking were fully appreciated by all concerned.

Another noncontractual effort, and which involved considerable Section time, was the advancing of the idea of gyro-stabilization of aircraft turrets. With gyro-stabilization of an aircraft turret, the gunner is enabled to follow the motions of his target with respect to space without regard to the angular motions of his own plane. The Section was convinced that it was just too much to ask of the gunner not only to track his target, but also to cancel out the angular motions of his own aircraft in the air mass. This selling job finally saw intense Service and independent company activity in the field of gyroscopic stabilization of aircraft turrets.

Other noncontractual activities in the gyroscope field occupied the Section. Assistance was given the Navy and some of its contractors in connection with the development of improved "stable elements" for use on shipboard and in aircraft. General consultation with the Services on gyroscopic matters was a frequent occurrence, particularly in connection with the development of gyroscopic lead-computing sights.

A recollection of the technical scene requires one to return to the history of Section D-2 where it was stated that the initial project contracted by that section was for research on servomechanisms.

In 1940 the position of the servomechanism with respect to the system of which it is a component was not universally appreciated. This was particularly so with regard to military applications, since secrecy considerations had resulted in piecemeal engineering of the complete system. Recognizing these facts, Section D-2 sought to undertake a substantial basic research program and accordingly initiated a contract

with M.I.T. for this purpose. During the course of the program, however, a part of the contractor's personnel was urgently needed for immediate Service problems under other agencies and this broad program was curtailed. This marked the beginning, however, of interest in stimulating writing and in sponsoring distribution of mathematical treatments of servo theory which resulted in the private publication and distribution of mathematical papers on servo theory by Gordon S. Brown, Herbert Harris, Jr., Albert C. Hall, and others of the Servomechanisms Laboratory of M.I.T. The recent book, *Fundamental Theory of Servomechanisms* by LeRoy MacColl of the Bell Telephone Laboratories, which was published openly after V-E Day, was the last of these papers written at the request of Division 7 and the Applied Mathematics Panel of NDRC.

Section 7.3 was responsible for several servo developments initiated in response to particular needs. Several of these have been mentioned in the history of Section D-2. Of these, three bearing upon the problem of fire control for medium-caliber antiaircraft guns resulted in a servo which was standardized by the Ordnance Department. Since this important development gives an insight into the Division's method of operation and as it represents a typical instance of co-operation, its history will be recorded in detail.

In the early part of 1941 the Ordnance Department of the United States Army adopted the British fire-control system known generally as the Kerrison System for the control of United States Army medium-caliber (37- and 40-mm.) antiaircraft weapons.

The Kerrison System comprises essentially an electromechanical director for tracking the target and predicting the lead angles, and a hydraulic-power unit for orienting the gun in azimuth and elevation. The director uses elevation and azimuth tracking and an estimation of range for prediction. The hydraulic-power-drive unit is actuated by a selsyn-type data-transmission system, the transmitter of which is driven by the director.

At the time of the adoption of this system by the Army Ordnance Department, it was recognized that the director and servos for the gun were not ideal, nevertheless there was no doubt about the wisdom of adopting the system. Then, as the problems incidental to production by American methods were recognized and overcome, studies of the com-

ponents of the system with a view to their basic improvement were given impetus.

Section D-2 was one of the groups officially interested in sponsoring work on improvements in the system. Early in 1942, the Section requested the Servomechanisms Laboratory at M.I.T. to undertake a general program of study and investigation of the properties of the hydraulic-power units.

Paralleling the program of improving the original design, a program was undertaken to develop a new servo system for the 37- and 40-mm. Army gun mounts. The new units were to have considerably greater torque, much smaller dynamic error, and be physically interchangeable with the original units.

At about this time the Servomechanisms Laboratory was completing a study of a small adjustable-stroke hydraulic pump and fixed-stroke hydraulic motors recently developed by the Oilgear Company of Milwaukee for use in gun and turret controls in medium tanks. Preliminary investigation showed that the pump and motor, then in production by the Oilgear Company, could be made to serve effectively as the principal hydraulic elements in the new remote-control system.

The Servomechanisms Laboratory was requested to undertake the design and construction of pilot models of a suitable replacement remote-control system using the pump and motor developed by the Oilgear Company. This work was accomplished, and pilot models of the substitute control were tested at Aberdeen Proving Ground and at Camp Davis. The hydraulic servomechanism was identified by the type number T-15.

Concurrently with this development, the Barber-Colman Company had been carrying on the development of clutch-type servos for medium-caliber guns. Accordingly, Division 7 negotiated a contract with the Barber-Colman Company for the continuance of this development under NDRC auspices. A servo embodying some unusual features was developed under the contract which served as an alternative means of mechanization to the methods chosen by the M.I.T. group.

The Westinghouse Electric and Manufacturing Company had accepted a contract to continue the development of the Servomechanism T-15 developed by Servomechanisms Laboratory together with the alternative clutch-type servo developed by the Barber-Colman Com-

pany. One consideration in choosing Westinghouse to develop the production version of the Servomechanism T-15 was because they were manufacturing the then standard servomechanism and such a contract would effect a continuity of over-all production facilities in the event of a change of design as a result of this development. The designs were modified where necessary to adapt them to production manufacture, with the object of interesting the using Services in adopting the design.

In August of 1942 a joint conference of representatives of Frankford Arsenal, The Oilgear Company, M.I.T., Westinghouse, and Section D-2 decided that Westinghouse should begin the preparation of production drawings of the T-15 Servo under its NDRC contract, and a new contract would be negotiated between the Frankford Arsenal and Westinghouse to cover the construction of 100 sample units and the preparation of production drawings in Ordnance form. The Ordnance Department standardized the T-15 hydraulic servo as a Remote Control System M-9 (for the 37-mm. gun mount) and M-10 (for the 40-mm. gun mount). By January 1944 tooling was completed and delivery of the first production units was made.

Another project which strictly speaking belongs to the history of Section D-2 was for the development of a seacoast data-transmission system. This project also reflects the excellent co-operation afforded NDRC by industrial laboratories and particular Service groups, and since it was a development sponsored by the group which was subsequently to become Section 7.3 its history will also be cited in some detail here:

In March 1941, the Coast Artillery Board initiated a project with NDRC for the purpose of obtaining a satisfactory solution to the problem of continually transmitting data from base end stations to gun-data computers. It may be of interest that the cable over which the data were to be transmitted was specified as a maximum length of fifty miles and of ordinary, and even rather "leaky," telephone line. A contract was negotiated with the Bell Telephone Laboratories for this purpose and a system was developed. Subsequently, a contract was negotiated with the Leeds and Northrup Company for the development of pilot models of this system. The pilot models were tested and accepted by the Coast Artillery Board. Various components of the transmission sys-

tem were standardized and these instruments are now a part of our coast defense batteries.

It will be remembered from the history of Section D-2 that its members gave considerable attention to the problem of gyroscopic lead-computing sights, and a particular contribution of Section D-2 was in the form of several theoretical studies bearing upon the problem. These studies subsequently led to Section interest in basic improvements in such computing sights and thought was given to a reconsideration of the problem of mechanization. In exploring methods of mechanization, attention was focused on pneumatic controls, which had long been used in industrial instrumentation but had not been exploited in the fire-control field. It appeared that important advantages would result from a lead-computing sight mechanization which involved the constraint of gyroscopes by pneumatic means. Accordingly, a contract was placed with the McMath-Hulbert Observatory and a problem set up under an already active contract with the Eastman Kodak Company for such a pneumatic development. These researches initiated a program of pneumatic instrumentation in the fire-control field and until termination of hostilities Section 7.3 was to devote substantially its entire effort to this field.

The Spring of 1942 found German submarine activity against Allied shipping so great and so effective that ships were being sunk at a rate greater than we could build them. This situation called for an urgent program on countermeasures. Work was proceeding at the Franklin Institute, under the auspices of Section D-2, and later Section 7.2, for the development of bombsights for antisubmarine warfare. In view of the new pneumatic techniques for fire-control devices already under way, Section 7.3 undertook the development of a bombsight embodying these principles and designs. The successful conclusion of this development was the result of a rather remarkable co-operative effort recounted in more detail below.

The urgent need for an antisubmarine bombsight resulted in the adaptation for this purpose of certain basic components under development at the McMath-Hulbert Observatory. A bombsight was accordingly hurriedly built and given preliminary flight tests by the Navy. These tests proved especially gratifying and the development program was vigorously pursued. Anticipating a production program by the

Navy, the Eastman Kodak Company was asked to initiate a production design of the bombsight under an already active Division 7 contract. An additional contract for the development of a precise speed control for aircraft motor generator sets was set up at the Leeds and Northrup Company since the bombsight required accurate control of the rotor speed of a gyroscope.

The continued development in the laboratories of these contractors in co-ordination with the program of flight testing by the Navy resulted in the standardization by the Navy of the sight as the Bombsight Mark 23. The drawings and other data necessary for specifications were turned over to the Navy and the Navy production program was initiated. But the co-operation did not terminate with the mere transfer of drawings and data to the Navy. The techniques were novel and, indeed, many months had been spent in their development. The personnel of the contractors and the Section were heavily drawn upon by the Navy and its contractor, and these groups along with the Bureau of Ordnance functioned as a single team in this development and production program. The personnel of these contractors worked together in such harmony that it was hard to realize that they were not housed under the same roof, notwithstanding that physically the Observatory was in Pontiac, the Eastman Kodak Company in Rochester, the Leeds and Northrup Company in Philadelphia, and the Section 7.3 offices in Washington.

In view of the successful application of pneumatic techniques to bombsights and also to lead-computing sights, it was desirable to expand and broaden the work in the pneumatic field. A contract was placed with The Foxboro Company for the development of an improved depth-control mechanism for torpedos. Also, a broad program was set up and a contract placed at the Lawrence Aeronautical Corporation for the development of compressible fluid controls for fire-control application. Four groups of projects were undertaken under the latter contract, three of which were supplemental to the work done under other contracts. One was in collaboration with the contractors developing the Bombsight Mark 23. Another was for an aerial camera stabilizer developed in co-operation with a Division 16 program for development of cameras and camera stabilizers. The third was in collaboration with Division 6 of NDRC, in connection with pneumatic

instrumentation for torpedo controls. The fourth group was for miscellaneous instruments using pneumatic means.

These efforts resulted in instruments which were in the course of final development at the termination of hostilities. All were turned over to interested Service groups for completion.

CHAPTER VII

OPTICS AND THE OBSERVER

WITH THE reorganization of Section D-2 as Division 7, that portion of the fire-control activities having to do largely with optical ranging instruments was assigned to Section 7.4. For those working in the Section, the only effect of the change was one similar to the beginning of a new year — one had to remember, for several days, to write 7.4 at the bottom of letters instead of D-2. Otherwise the work went on without interruption.

The Section originally consisted of T. C. Fry, Chief, P. R. Bassett of Sperry Gyroscope Company, T. Dunham, Jr., and S. W. Fernberger, the latter also serving as the Technical Aide of the Section. In December 1943, Fry resigned to return to industry and to devote all his free time with the Applied Mathematics Panel of NDRC. Bassett was made Chief of the Section without other change in personnel. In passing, it is of interest to note that the Section never held a physical meeting and, indeed, two of the members would not recognize each other if they should meet accidentally. However, there was close cooperation among the Section members by individual personal contact or by correspondence.

Section 7.4 was to continue much of the research activity initiated by the former organization. These activities were divided into six classes: (1) Producing the best possible operators by selection and training; (2) Determination of the best possible methods of operation; (3) By fundamental studies, the determination of the limits of human ability and the best man-machine combinations; (4) Making the available optical ranging instruments give the maximum accuracy of performance by elimination of instrumental errors; (5) The development of new instruments and of more stable instrument parts; (6) The adapting of optical ranging instruments to new military patterns and new military situations.

It will be recognized that categories 1, 2, and 3 are largely or entirely psychological and physiological in nature; categories 4 and 5 are devoted

to instrument engineering; and category 6 contains both psychological and engineering elements. This last subdivision is perhaps one of the outstanding contributions of the work of both D-2 and 7.4—the recognition that one must study the man-machine combination, not either one in isolation. The greatest possible efficiency can be achieved only if engineers and psychologists work closely together in producing a machine which is to be humanly operated. In this connection it may be of interest to quote from a letter from Bassett, an engineer and President of the Sperry Gyroscope Company, to Fernberger, a psychologist. He says in part: "I have learned more from the psychologists than from the engineers, and I hope that my experience is that of many others since I think we all had much to learn from our neighboring sciences." This co-operative pattern was more and more fully recognized elsewhere in NDRC as time went on and also by some, but not all, of the military personnel with whom the Section dealt.

Due to the nature of the problems investigated, 7.4 turned out more paper than hardware. Obviously this would be the case in a field where so many problems were of an operational rather than of a structural sort. From January 1, 1943, through December 1945, the Section distributed thirty-four reports to the Services, or at a rate of almost one a month during the three-year period. There were many more contractors' reports than this, because many of these reports to the Services were summarizing in nature and each covered a considerable number of contractors' reports. Also, many reports by experimenters to NDRC were never distributed to the Services because the results were either inconclusive, or of such a minor magnitude or importance as to be of no practical value. The percentage of such "failures" was relatively small. Indeed, many proved to have positive value inasmuch as they indicated that factors formerly considered of significance by the Services turned out to have slight influence only and could be disregarded. In this latter category are the splendid laboratory experiments by the Ohio State University on the effects of haze on ranging accuracy. The results are completely positive and the errors are much smaller than those introduced by other known factors. The minds of Service personnel could therefore be made easy in regard to the difficult operational procedures necessary to correct this effect—for the time being at least.

To begin the recitation of specific accomplishments, it is fitting to

begin with the story of the Section's interest in problems of selection and training, surveying first the general picture and then returning to the individual jobs which made up a complex maze of a gigantic co-operative effort.

The Princeton Laboratory had been set up by D-2 at Fort Monroe and 7.4 continued this activity. Fort Monroe had been chosen because this was the location of the only training center for Stereoscopic Height Finder Observers for the Army Antiaircraft Command. Here enlisted men could be studied during their training and the experimental data obtained could be validated against their accomplishment and their class standings.

Almost immediately after Pearl Harbor, the Army had pressed the Section for a group of selection tests, even though those suggested could no more than provide an educated guess. This was done reluctantly in February 1942, although the Section pointed out clearly that the suggested selection procedure had not been validated against trained ranging performance.

The guess proved to be a good one and stood up under subsequent validation. One year later, a manual for the selection of range- and height-finder observers was circulated to the Services. The Army promptly adopted the selection suggestions — the Navy adopted them at a considerably later period.

The validation of these tests was slow because of the small number of personnel under training for this highly specialized job and because, due to the need for trained personnel to operate equipment in the field, the School could accept only those individuals who gave promise of real success as the result of training. The results show that the tests were capable of picking the 3 per cent of the total Army population who gave greatest promise of becoming adequate operators.

To point to the order of magnitude of the intrinsic difficulties experienced in this psychophysiological field, we might parenthetically examine one of the stumbling blocks in the development of the test battery, namely, the evolution of means adequately to measure stereoscopic acuity. Many tests were tried, one of them being the Vectograph Test mentioned among the projects of Section D-2. Few of the tests stood up under validation against subsequent ranging ability. The Vectograph Test was even tried with aviation-pilot candidates at

Randolph and Kelly Fields and at the Philadelphia Navy Yard, but without success. A concurrent effort was through a contract with the Psycho-Educational Clinic at Harvard University, but this group failed to produce an adequate and usable selection test of stereoscopic acuity. Finally a test was developed at the Princeton Laboratory, with the assistance of the Dartmouth Eye Clinic, which proved satisfactory and which was further improved by subsequent work. The outcome of this composite effort with tests of stereoscopic acuity was the interesting and valuable finding that no static test could be validated with subsequent dynamic activity. Hence a dynamic test was indicated and the ultimately accepted measure was of this type.

The Army set up elaborate selection testing centers at the several Antiaircraft Replacement Centers so that thereby enough men arrived at the Height Finder School who were apt to become adequate operators through training. When the Antiaircraft Command was divorced from the Coast Artillery, it seemed that the Princeton Laboratory had already made its greatest contribution and that, from then on, there would be diminishing returns from this research group. It was then decided to close the Princeton contract and to transfer the selection and other phases of the work to other auspices. About this time there was formed the National Research Council's Committee on Personnel Selection and Training, and all matters in the fields of selection and training were turned over to them for further development. The work of this Committee was in turn absorbed by the Applied Psychology Panel of NDRC when it was organized.

Still another co-ordinated selection study was carried on by Brown University on the difficult task of developing a test of emotional stability under stress. At one time a skeleton battery was tested at Fort Monroe. This battery was to proceed to England and be located near a British battery so that the two antiaircraft fire-control systems could be given a comparative test under combat conditions. In this way, a validation of the emotional tests was hoped for even though the sample was small. Unfortunately the scheme fell through and the battery was never sent abroad for this purpose. This problem was also turned over to the NRC Committee and eventual validation of an often revised series of tests was accomplished with submarine personnel.

The work of the Fort Monroe Princeton Laboratory on the problem

of training stereoscopic range-finder operators was undertaken because of three circumstances: —

1. Just before the opening of hostilities, and in the early stages of the American phase of the war, this training problem was extremely acute particularly for the Antiaircraft Command. Radar had not yet been perfected. Hence the only means of determining range to an enemy aircraft was with optical instruments, and there were all too few of these available. The Height Finder School at Fort Monroe, at that time the only training center for operators of these instruments, never had more than seven stereoscopic height finders available at any one time. And no additional instruments could be procured for training purposes because new batteries were being activated faster than new supply was received from the manufacturers. One substitute training instrument existed but this was inadequate, except in the very early stages of training, because it did not present the kind of dynamic situation to the observer that he would subsequently meet in the field.

2. The Services did not know the criteria of excellence of performance either desired or to be expected from trained personnel. Both Services had a criterion of excellence of one Unit of Error, defined as an error of 12 seconds of parallactic angle at the eye. It was found that this level of performance could not be maintained by highly selected and trained test observers against a real target, even when flying accommodatingly planned crossing courses.

3. The training course consisted of a 17-week period of study.

The Princeton Laboratory managed, as records piled up from class to class, to determine realistic criteria and a new set of graduation requirements was established. Too, the existing training instrument was modified by the introduction of dynamic courses and, by working with the Eastman Kodak Company, a new and greatly improved training instrument was developed. This new instrument was immediately adopted by both Services and, indeed, adopted so enthusiastically that a conference had to be called to determine priorities for delivery among the different Services.

It was found, working with the actual classes, that this instrument could be used alone during the first half of the training, and observers so trained reached as high a level of proficiency by graduation as those trained completely on the field instruments. Hence approximately twice

as many could be trained by running two classes simultaneously, in staggered fashion, with no increase in field equipment.

A study of learning rates by the Princeton group, and a revision of the curriculum, enabled the course to be cut from 17 weeks to 12 weeks, with as adequate preparation resulting as heretofore. And, with proper selection, a far higher percentage of entering students reached graduation criteria than under the older system.

The range-finder training program produced an interesting by-product, a comparative study made of sex differences in several laboratory studies of ranging and tracking made for other purposes, which disclosed that women were as efficient as men for both stereoscopic observing and for direct tracking. It was thought worth while to have this fact established beforehand if there should be a manpower shortage, such as had occurred in England where women were utilized for these activities in the field.

These studies of selection and training constituted a substantial program. It certainly could not have been accomplished except for the enthusiastic and complete co-operation of Colonel A. L. Fuller, Jr., then Director of the Fort Monroe Height Finder School, and of the members of his instructional staff.

A series of studies was made at the Dartmouth Eye Clinic to determine the prevalence of aniseikonia in the general population. This condition of unequal images in the two eyes, if present, would seriously affect ranging accuracy. A further study was made to determine if there were individuals who exhibited aniseikonic vision when fatigued but who could compensate for the condition in the normal rested state. The results of both studies proved negative and were never reported but the findings were valuable in that the negative result was now known.

Another field of studies at Fort Monroe had to do with operational procedures for stereoscopic ranging instruments. These covered a wide variety of topics such as methods of bracketing, relative position of target and reticle lines, change of power of the eyepieces, the effects of motivation and fatigue on ranging accuracy, the effects of change of position, of hyperventilation, drugs, the setting of the eyepieces to the interocular distance of the observers, the effects of head position, and the like. Studies on many of these problems were carried on simul-

taneously at several universities under strict laboratory conditions, namely, at Harvard, Tufts, Ohio State, and Dartmouth. Some of these factors turned out to be important, others totally unimportant. The studies led to many recommendations to the Services—some of a negative character. For example, the Services could be assured that there would be no decrement in ranging accuracy for short periods of highly motivated observation even under conditions of great fatigue. The presence of an enemy aircraft within shooting distance could be expected to supply the required motivation.

The composite program was still furthered by systematic studies made at Bausch and Lomb of the effect of power change in the objectives. These led to the recommendation for single rather than variable-power eyepieces. The problem of the correct interocular setting of the objectives proved to be of great importance. Studies were made at Bausch and Lomb and at Harvard University. The Harvard study indicated that, under certain conditions, another measure was preferable to the usual interocular setting. This is because the principal lines of sight do not always correspond to the centers of the pupils. The Harvard group developed a bench model of an instrument for determining this new measure. The Navy had several copies engineered by a commercial company. Unfortunately the manufacturer incorporated certain errors of construction which made them practically useless. Inasmuch as the interocular distance was found to be the best setting for many situations, it was recommended that this be employed at all times. One outcome of these studies was the realization that the interocular setting must be measured very exactly for each observer, and that the observer must take great care in introducing the correct setting into the instrument. Another outcome was the development, by Harvard, of a self-locking device for the eyepiece assembly. An interocular setting, once introduced, could thus not be changed by the operator pressing against the eye cups and so spreading the objectives. This and other suggestions were adopted by the Services, and the introduction of the self-locking device went into immediate production.

Section 7.4 attempted the development of short-base range finders which could be hand-held and which might prove useful for the fire of small-caliber antiaircraft guns and for many other applications. Under contract, instruments of this type were developed by the Eastman

Kodak Company and the Polaroid Corporation. Both instruments were delivered at about the same time and were given field tests at an Antiaircraft Battery position on Long Island through the co-operation of Captain Getzinger, the Battery Commander. Enlisted personnel, both unskilled and the Battery's trained observer's, were used as subjects.

Neither instrument proved of sufficient accuracy to indicate recommendation for acceptance. However, the lessons learned enabled the Eastman Kodak Company to develop very rapidly a ranging instrument which was incorporated in a director for the control of medium-caliber antiaircraft guns, and standardized by the Army as the M-5A2.

With a probable change of emphasis from defensive to offensive warfare, Section 7.4 set about adapting range finders and fire-control devices to new offensive situations. This change of attitude was already evident in a letter from Thornton C. Fry to Warren Weaver, dated April 16, 1942, which reads in part:

"I don't think Conant's phrase 'altered tactical situation' was intended to awake these thoughts in my mind but it did:

"The impact of a research program upon society is always delayed. In normal peace-times the delay is pretty long, not often less than five years and sometimes, as in the case of the automobile, a good quarter of a century. In war, the process is speeded up, but certainly cannot average much under two years.

"The defensive phase of this war we are fighting is going to be fought out on the basis of research already done. If the defensive weapons now in service or already thought out and about to go into service are not good enough to stop the Germans and the Japs, the war will be over before any others not now in the mill can be developed. If the ones now in the mill are good enough to stop the enemy, the next phase will be an offensive war.

"Ergo, our activities ought to be concentrated almost exclusively in two categories: (1) the reduction to hardware of those defenses which are already well past the idea stage, and (2) the search for more effective offensive weapons.

"To date Section D-2 has done almost nothing in the second field. Your work on the bombardier's calculator, and Caldwell's on the torpedo director are virtually the only examples. What say we begin to

cultivate an acquaintance with Infantry, Artillery, and Mechanized Land Forces."

Following this suggestion, Section 7.4 chose the tank for the first application. Up to 1943, the American Services had merely employed visual estimation of range and the usual artillery-bracketing technique with tanks. It was hoped that the use of a range finder might enable the tank to fire for effect with the first round. Reports, at this time, indicated that the British in North Africa were in this way utilizing captured enemy range finders with excellent results. The experience of the Section with the antiaircraft application was of not very great assistance in this new application because of differences in the ranges involved, size limits of the instrument, backgrounds, camouflage of the targets, and the like.

Hence a contract was negotiated with Bausch and Lomb to make an exploratory study of the comparative value of available instruments in this situation. The instruments selected represented different types of viewing field and they were taken to Fort Knox for testing. Through the co-operation of Lieutenant Colonel L. T. Heath, Major F. S. Brackett, and Captain I. H. Sclater, enlisted personnel were made available as observers and a terrain with suitable targets, including vehicles and tanks, was reserved for the purpose.

The results of this study indicated that personnel trained in visual estimation were more accurate than those untrained, but that instrument ranging was far superior to even the best trained estimation. As a result of the field study, the Armored Force Board adopted a particular instrument for use, partly because it had shown up well in the tests and partly because of immediate procurement availability — partly from Canadian and partly from American sources. Another outcome of the experiment was the decision of Lieutenant Colonel A. F. O'Meara to introduce training in range estimation as part of the regular program of the Armored Force Gunnery School.

As a result of these field tests, 7.4 suggested that they proceed with the development of an automatic self-contained fire-control system in the tank, with the range finder acting as ranging instrument and as sight and with automatic positioning of the gun through selsyns. The newsreel pictures of a tank bowling along over rough terrain and firing continuously is completely Hollywood. Such action depends on the

stabilization of the gun, which was completely in the experimental stage at the time of these experiments. However, such satisfactory stabilization was promised for the near future and would involve redesign of the tank turret and chassis.

The Section hoped to have the fire-control system completed in time so that a single redesign would include both sorts of modification. A conference was held early in 1943 at which the Army Ground Forces did not support the suggestion, and it was accordingly dropped.

At Navy request, The Foxboro Company performed a series of laboratory experiments comparing a variety of machine-gun sights for such operations as slewing, central and lead tracking. This same Company also performed a field experiment with tracking and simultaneous stadiametric ranging of incoming planes. This was followed by a series of studies in the laboratory which considered such matters as reticle design for stadiametric sights, and systems of tracking and ranging controls. These studies were a great aid to the Navy in shaping design policy for sights for machine and light-caliber guns. The advice and co-operation of Commander S. S. Ballard was of the utmost value in these researches and, indeed, throughout most of the work of the Section.

We turn to another important series of problems, faced by 7.4, namely, the development of optical ranging instruments superior to those then existing. This work seemed of the utmost importance even though it was more than possible that the new instruments could not be designed, built, tested, and in production by the end of the war. However, this seemed the opportunity to carry on this project at a time when scientific, manufacturing, and military personnel were co-operating to such a high degree, and when ample funds were available. Certain fundamental studies were therefore set up to obtain the necessary basic information.

One primary problem had to do with the type of instrument and its field of view. Optical ranging instruments fall into two classes, the stereoscopic (where both eyes are used simultaneously), and the coincidence (where viewing is with a single eye). Many variations and sorts of field are possible in both types of basic optical system. A contract was set up at the California Institute of Technology to test the acuities which might be expected from the various types of optical system. This

group produced an extremely ingenious device by means of which it was possible to switch, almost instantaneously, from one type of optical system to another. The results were conclusive in favor of the higher acuities of the stereoscopic system.

Since the Battle of Jutland in World War I, the British and American Services have argued which type of range finder should be used. The British had adopted the coincidence type of instrument and the Americans the stereoscopic type. In order to test out the matter under semi-field conditions, the British sent a naval instrument and its crew to Fort Monroe to be matched against an American crew and their instrument. Both crews were specially trained on both types of instrument and each team was to alternate between the instruments during the tests. However, weather conditions delayed the completion of the comparative test, which had to be terminated when repairs were completed on the British ship and the instrument and its crew had to be returned. Hence the comparative test was inconclusive.

Among other fundamental studies, by The Foxboro Company, was the analysis of handwheel tracking. When it had been discovered that the positioning of target and fiducial reticle line was of great importance, the matter of tracking accuracy was emphasized for stereoscopic range finders of the reticle type. The Foxboro group were able to analyze the influence of such basic factors as handwheel speed, learning rates, diameter of the wheel, inertia, friction, magnification, and use of one or two hands. It is doubtful if anyone should design an instrument in which direct handwheel tracking is involved without careful consideration of the influence of the fundamental factors discussed in these reports. The Foxboro Company also reported a study comparing direct and aided handwheel tracking.

Still another set of fundamental studies, which had to do with reticle design, was performed by Brown University. This included such matters as effects of imperfections in the reticle field and a comparison of existing reticle patterns with new patterns suggested from several sources. From these studies a number of basic principles were evolved which must be the basis for all future reticle design. It was subsequently found, by the Brown University group, that exactly the same principles applied to illuminated or projected reticles as had applied to those of the opaque sort. Brown University devised a simpler reticle design, different from

any of the present Service types, which gave the best acuities under laboratory conditions and also avoided certain sources of error found in Service reticles. This pattern has been produced and is under test by the Navy.

At the very close of the Brown University program, they had developed a method for studying atmospheric "boil" and the relation of this to the base length of stereoscopic instruments. With V-J Day and the closing of the project, a sufficient number of results could not be obtained to indicate more than the adequacy of the method and to give an indication of the critical base length.

An important fundamental study was carried on at Harvard University. This consisted of an analysis of the physiology of the ranging process under both normal (unaided) and telescopic viewing. The work was carried on in the laboratory and in the field, where realistic ranges were used both over flat terrain and over water. It was found that the normal ranging processes are based upon three fundamental cues: parallax (resulting from disparate retinal images), size changes, and wave-front change (resulting in accommodation changes in the eye). Of these, parallax is the major factor but the other two are by no means negligible. It turned out that judgments of distance were impossible if all three cues were eliminated, and it was also found that these three cues are additive in their effect. The results of the initial studies indicated that greater acuity in range estimation was obtained with unaided vision or with a simple binocular telescope than with the usual optical military ranging instrument.

In the present stereoscopic instruments, the observer can use only the parallax cue, although in the light of the Harvard results the other two cues are undoubtedly both present, each giving separate discordant contributions to the sensing of distance. That the successful operator must override these two in favor of the stereoscopic cue may help to account for the comparative rarity of good range-finder observers. Under great pressure of closing the contract after V-J Day, the Harvard group produced a bench model of an instrument which would utilize all three stereoscopic cues. It was impossible to test this bench model due to lack of time. The model was turned over to the Design Division of the Frankford Arsenal for development and testing of the fundamental design.

Let us now consider the matters of new design. About everything possible had been done to improve the existing instruments. It was recognized that the introduction of helium was only a temporary measure to obtain temperature stabilization. This fact became obvious from a Princeton Laboratory study of helium retention in which it was found, as expected, that some instruments leaked like sieves. The solution of temperature stabilization therefore required the development of temperature-stable parts.

This development was carried on by Bausch and Lomb and by the Eastman Kodak Company. Both companies worked on the use of invar for the optical bar with positive results. It is interesting to note that the best formula for the invar alloy, found by Eastman, was obtained from a Japanese publication. Eastman also studied temperature effects on mirrors. They worked out important and interesting results which will have much to do with the eventual temperature stabilization of the pentareflectors, variation in which is a very serious source of error in stereoscopic range finders.

Brown University performed experiments on the form of target for the internal adjuster systems of stereoscopic range finders. They suggested the introduction of the so-called Abbe adjuster system instead of the present binocularly viewed arrangement. The Navy modified an instrument but the results, although interesting, are inconclusive on the basis of the comparative test of only two instruments.

It was recognized that all of the several factors discussed just above might be introduced into present instrumental designs and that such introduction would result in improvement of ranging accuracy. However, improvement of any considerable amount can be expected only by complete and fundamental redesign of the ranging instruments.

One such fundamental redesign was completed quite early in the history of Section D-2. Hence a comparative test was made at Fort Monroe between a normal instrument of the stereoscopic reticle type and this other stereoscopic instrument, but without reticles, which had been independently developed by the Eastman Kodak Company. No considerable difference could be found between the two types of instrument although, theoretically, the new type should have had twice the acuity of the Service instrument.

A conference was arranged to discuss the complete new design of optical ranging instruments at which were present representatives from both Services, Bausch and Lomb, Eastman Kodak, Keuffel and Esser (the three manufacturers of these instruments), and NDRC personnel. The three manufacturers were invited to submit new designs. At a second conference, several such designs were selected for development from among the number offered. Some of these were near completion by V-E Day. Shortly thereafter the contracts were taken over by the Army and Navy.

This last item is perhaps the most hopeful sign in the optical range finder situation. It is the first time that the three manufacturers have even sat down together and exchanged information. Indeed, the representative of one firm was heard to remark facetiously, when a conference was held in another firm, that he was surprised that he was not blindfolded when he entered the factory. It was the first time that any member of either company had ever been inside the factory of the other firm, although both were located in the same city! It would seem that the very frank exchange of information, experienced during the period of NDRC development, must have a highly beneficial effect on the development of the art.

In summary of the Section 7.4 history, it would seem that there is a mixture of success and failure, but that the proportion of success far outweighs the proportion of failure. Certainly it was worth while to develop any project which might seem to be important, even if to determine that the factor studied was unimportant from a practical point of view. The existing optical ranging instruments were brought to as high a degree of efficiency as possible, if placed in adequately competent hands. No great factor of increased efficiency can be expected in optical range finding by a mere modification of the existing instruments, although some improvement is certainly possible. But considerable improvement may be expected only through complete fundamental redesign.

This is the day of great publicity for radar, which seems to make unjustifiable the expenditure of any considerable time, effort, and expense for the further development of optical ranging instruments. But there are many informed individuals, both in and outside of the Services, who

believe that the fire control of the future may be primarily optically directed because of the possibility that effective radar countermeasures may be developed.

The last Report to the Services from Section 7.4 was circulated on December 15, 1945. Six days later the resignation, from NDRC, of the Technical Aide became effective and the 7.4 office was closed.

CHAPTER VIII

MATHEMATICAL ANALYSIS OF FIRE-CONTROL PROBLEMS

AT THE reorganization of NDRC, the Applied Mathematics Panel was set up to provide mathematical analysis as needed by the various new Divisions of NDRC and by Army and Navy groups. Warren Weaver, as Chief of the New Panel, remained a Member of Division 7 and also served as Chief of its Section 7.5 devoted to the analysis of fire-control problems. In these various capacities, Weaver was assisted by Hallett H. Germond, who was appointed Technical Aide of Section 7.5, as well as by the members and Technical Aides of the Panel. This Section took over supervision of the Section D-2 contracts in its field and later initiated new projects. Many of the contracts provided the services of mathematical groups. It later proved useful to transfer the administration of the contracts to the Panel, in order that these groups could work flexibly on the problems of the Panel regardless of their divisional or Service origin. In what follows, an account will be given of some of the mathematical projects relating to fire control quite irrespective of whether the work was done by Section D-2, Section 7.5, or by the Applied Mathematics Panel.

It will be recalled that early emphasis was laid upon mathematical and theoretical analysis of various fire-control problems. One of the first fruits of such an attack was the issuance of two theoretical studies on lead-computing sights. This has been related in some detail in Chapters III and IV. The theoretical studies were prepared by members of Section D-2 and were not the result of a formal contract with some private group. There were, however, ten formal contracts of Section D-2 and Division 7, in the province of mathematical analysis. These projects fell into three major categories: contracts for the services of individual mathematicians; contracts which involved the development of mechanical aids for computation; and finally contracts covering broad mathematical services. Thus, in the first category, two projects

with the Massachusetts Institute of Technology drew particularly upon the services of Norbert Wiener and Julian Bigelow. Similarly, projects were placed with Princeton University and with the University of Wisconsin for the services of C. E. Shannon and I. S. Sokolnikoff. The results of these contracts were in the form of mathematical papers on various problems of fire control.

The second category of the Section's work points to relatively more concrete results, namely the production of, or improvement of, particular articles of "hardware"; something relatively easy to assess when compared with the more abstract mathematical papers mentioned before. These material instruments, or items of hardware, were certain types of computing machines. Mechanical computers exist in many forms, ranging from simple adding machines to elaborate punched-card accounting systems, and from slide rules to differential analyzers. Perhaps the most widely known of the more elaborate mechanical computing devices is the Bush differential analyzer.

The Moore School of Electrical Engineering, at the University of Pennsylvania, operated its differential analyzer on a full-time basis in the solution of the differential equations of exterior ballistics under direct contract with the Aberdeen Proving Ground. Because of the large amount of work to be done both at Aberdeen and at the University of Pennsylvania, it was desirable to improve the efficiency of this machine. It was hoped that, as a result, manufacturing designs and specifications could be prepared for replacement units which could then be procured directly by the Ordnance Department and the University of Pennsylvania. Thus Section D-2 initiated a project with the University of Pennsylvania for the improvement of their differential analyzer. At the conclusion of the contract such improvement was obtained and that computing machine was able to speed up its war work.

In several instances during the war, routine numerical computation became a bottleneck in the prosecution of testing programs. This situation naturally focused attention on the possibility of carrying out such computations by means of automatic calculating devices.

The efforts of the Section were directed toward the construction of computers using standard telephone relays as computing and control elements together with standard teletype punch tapes as auxiliary storage devices, and as the control which programmed the steps of the

computing. It was realized that electronic equipment held great promise in the high-speed computing field, but it appeared that the established techniques in the manufacturing facilities associated with relays would permit earlier completion of large-scale computing equipment. The history of the last few years seemed to have borne out this prediction. Thus, while electronic computers of tremendous speed have been built, their development did not reach the point of actual use until some months after the war was over; whereas three relay computers were built during the War, each in a span of three or four months. While these devices were not as rapid as electronic equipment, they too have been found to be highly reliable and useful developments.

In the design of large computing systems, it has been found desirable to dissociate the operator's function from those of the machine because of the difference of speed and working hours of these two components. The computers could be built to accept data previously punched on tape by one or more operators. The operators could work at their convenience as to speed and hours and the machine could be free to pick up data as required and to work twenty-four hours per day. Such an arrangement naturally simplifies the administrative problems and increases rapidly the capacity of the computing systems. Two such machines envisioned by Division 7 and built under the auspices of Section 7.5 were the Relay Interpolator and the Antiaircraft Artillery Board Ballistic Computer.

The relay interpolator was originally designed to accept a sequence of five-digit members (punched on an input tape), and interpolate between every two such numbers numerous (actually nineteen) intermediate or interpolated values, using in this process third-order difference formulae. This process was highly useful in connection with the computation of the various co-ordinates of an aircraft on either a straight or a curved path (antiaircraft prediction problem). Actually, and because of the extreme flexibility of relay computers under punched-tape control, this same device was used for a wide variety of other computing purposes, including the solution of differential equations, etc.

The second computer, the AA Board Ballistic Computer, was designed for the Board under a contract between the Division and the Western Electric Company. This computer can store ten six-digit num-

bers and will add, multiply, subtract, or divide. It has the additional ability to hunt up and use functions of one or more variables such as the ballistic data, and accepts problem data from two tapes. Its operation was controlled by means of formulae prepared in coded form on punched tape. The results appear in printed form, tabulated as the operator may desire. The primary purpose of this computer was to accept the actual experimental data obtained when an antiaircraft gun is test-fired against a moving aerial target (such as a towed sleeve); and to compute automatically from these data concerning gun position, target position, and the *errors* in the gun position.

In all large machines the problem of maintenance may be serious. It was therefore thought worth while to spend considerable effort in designing check circuits which would automatically stop the machine in case of trouble and would give the maintenance man fairly detailed information to help him diagnose and repair the computer. While it has been found that such troubles occur only one to four times per month on the average, still the check feature is invaluable.

The third broad category of projects involved rather extensive mathematical organizations, capable of attacking a variety of problems. In July 1941, Princeton University undertook a contract for the design of a bombardier's calculator. Subsequently this work, under the direction of J. D. Williams, was expanded to include a contract with the University of California under the direction of J. Neyman, and the Princeton contract was expanded and shifted to Columbia University. This contract subsequently developed into a broad study of the statistics of train bombing.

Basic tables for the probability of at least one hit for various proportions of rectangular targets were calculated, showing the way in which this probability depends upon the number of bombs in the train, the bomb spacing, the angle of approach, and size and proportion of the target; and the magnitude of the aim and dispersion errors.

Extensive studies were carried out to extend the results to produce a general theory of probable hits on multiple targets. Studies were also made to determine whether or not these theories could be applied under circumstances where (as will always be the case) the aim errors are not actually known. The various theories which were thus evolved were checked by a study of a train-bombing experiment at Eglin Field and also by an experiment carried out for this purpose on a bombsight

trainer. The study was extended to determine the optimum type of attack on maneuvering targets.

As a result of a conference held by the Fire Control Division, with representatives of the Naval Bureau of Ordnance, the Naval Bureau of Aeronautics, the Office of Co-ordinator of Research of the Navy, and the Army Air Corps, there was planned a general program for a series of probability and statistical studies of plane-to-plane fire. It was hoped that such a study, by evaluating the influence of the various factors on the over-all effectiveness of plane-to-plane fire, would give some practical information on certain problems of design of airborne fire-control systems. Accordingly, a contract for this purpose was negotiated with Columbia University.

In connection with the variety of projects and plans, it was increasingly necessary to have computations performed. These were both of routine and of highly specialized character. In each case it is most efficient to have the actual work performed at a particular place which is best equipped to handle it. A contract was made with the Franklin Institute to enable the Division to carry out computations whenever the necessity arose by requesting the Franklin Institute to procure the work from the most effective agency. The amounts of money involved were relatively small and the particular project could often be completed in less time than would have been required to negotiate a separate contract for the work. This contract proved exceedingly useful. Under it were supported the study of differential analyzer-ballistics problems, a study of fragmentation-damage problem, a study of scatter bombing, etc.

Early in 1943, the Columbia project was expanded in personnel and broadened in scope so that it could undertake any studies of the general title of "Air Warfare Analysis." The group was then composed of about fifteen technically trained people. Among the studies that were undertaken as a result of direct request from the Services or other NDRC Divisions were statistical acceptance tests for bombsights; analysis of the dive bombsight; estimate of additional risks of a bomber due to extensions of a straight bombing run; probability of damage to a dive bomber; optimum ammunition for air combat, and counter-evasion measures for aerial torpedoing; problems bearing upon the statistical aspects of the testing of certain Naval Antiaircraft fire-control equipment; probability of damage to aircraft through AA fire, plane vul-

nerability, optimum interrelations of aiming errors and gun dispersions; etc., etc.

Two projects in Air Warfare Analysis deserve special mention. One was a study of the relative effectiveness of two types of fighter-plane armament in attacking a defended bomber. The study was necessarily inconclusive, for the data on vulnerability were incomplete. The analysis showed the relative advantages of the two types of armament for each of a series of assumptions as to vulnerability and pointed out the exact nature of the vulnerability data necessary to give definite answers. The nature of the tentative answers was such as strongly to suggest which of the two types would be found superior when complete vulnerability data were available. This study was a clear illustration of the fact that proper statistical methods will often give important results even from very incomplete data.

The other project to be mentioned was a quite extensive study undertaken somewhat reluctantly by the Applied Mathematics Panel. Its objective was nothing less than a study of the most effective tactical application of the B-29 airplane. This project was requested by the Headquarters of the Army Air Forces and substantial numbers of aircraft were assigned to the work. The experimental program, carried out chiefly at Albuquerque and Alamogordo, began early in July 1944 and the main results were reported to the Air Forces on November 15 of the same year. It is of interest to note that 1448 separate airplane flights were carried out (all without accident) including 348 flights of B-29 craft. Over 130,000 feet of 16-mm. film was exposed in gun cameras. A total of about 350 persons were involved in the whole project.

An interesting portion of the B-29 study was the development of an optical method of simulating fire power. This was carried out by the Mount Wilson Observatory of the Carnegie Institution of Washington. In it squadrons of model planes could be set up, in various formations and their fire power determined in various directions. The method proved very successful, both for visualizing formations and for quantitative studies of fire power. A set of models was sent to the Marianas for the use of the 20th Air Force.

These two projects have been mentioned to show that fire-control analysis, to be effective, must extend beyond the design of weapons to include studies of their optimum use.

CHAPTER IX

NAVAL RADAR FIRE CONTROL

SECTION 7.6, Fire Control with Radar, was organized in January 1944, too late as it proved to make a direct contribution to the war. I. A. Getting of the M.I.T. Radiation Laboratory was Chief of the Section and its Members were George Agins of the Arma Corporation; R. E. Crooke of Ford Instrument Co.; C. S. Draper of M.I.T.; A. W. Horton of the Bell Telephone Laboratories; R. M. Page of the Naval Research Laboratories; E. J. Poitras of Division 7, NDRC; R. B. Roberts of Section T, OSRD; A. L. Ruiz of the General Electric Company (and already a Member of Division 7). This group was brought together in an attempt to co-ordinate the somewhat scattered activities in NDRC (and OSRD) relating to radar fire control, particularly for naval purposes. Some background material may be useful in assessing its activities.

There is no question that at the outset of the war the United States Navy had available better fire-control equipment for main battery control, as well as for long-range antiaircraft control, than the next two largest sea powers, Great Britain and Japan. This superiority can be traced very largely to the skill of the private corporations working with the Bureau of Ordnance: the Ford Instrument Company, the Arma Corporation, and the General Electric Company. Much credit is due of course to the officers in the Navy who had sponsored these developments and saw to their adoption by the Navy. All of this work was carried out under the protection of security regulations which, together with natural commercial rivalry, tended to minimize the exchange of ideas between the various company groups. A number of circumstances conspired to make the Bureau of Ordnance somewhat unreceptive to new technical groups which might seek to enter the field. The equipment available at the beginning of the war was of good quality; the operation of security regulations had prevented other groups from gaining the intimate knowledge of naval fire-control policies to qualify them as "experts"; and any changes would require a major problem in

installation in ships, a long training program, and the stocking of depots in many parts of the globe. As has been suggested in Chapter III, the original NDRC group in fire control, Section D-2, turned its attention in antiaircraft development to Army problems, largely because the need was greater but partly because their offer of co-operation had been accepted so enthusiastically by the Coast Artillery Board.

The introduction of the Draper Sight (Mark 14, for which NDRC can claim no credit) may at first seem to refute this charge of conservativism against the Bureau of Ordnance. But it must be remembered that no satisfactory equipment existed previously for this problem, lead computation for automatic weapons.

In contrast to this relatively well-established field, in which policies tend to become conservative, new fields opened up in which the Navy cordially welcomed early NDRC participation. We have already seen in Chapters III and V what effective support was given by the Navy to NDRC efforts to develop equipment for airborne fire control. Other new fields in which there was effective collaboration between the Navy and NDRC included microwave radar and, as we shall see in succeeding chapters, proximity fuzes. It was really through these two new areas that participation in the Navy fire-control program arose.

NDRC activities in radar were centered in the Radiation Laboratory at the Massachusetts Institute of Technology, operating under a contract supervised by Division 14 of NDRC. This group developed a 3000-megacycle fire-control radar in 1941 for use with the fire-control Director Mark 45. This early microwave radar was put into production by the Bureau of Ordnance and formed the basis of all the antiaircraft microwave sets which the Bureau procured. The production designs had been made by a contractor of the Bureau. Little benefit was derived from any of these radars because none of the directors with which they were associated was put into use for one reason or another. In the meantime, a strenuous effort was made by the Radiation Laboratory to determine the desirability of providing radar range to the lead-computing sight, Mark 14 (Draper Sight — September 1942). While the development was successful, the Bureau ruled that the complexity introduced by the radar range did not sufficiently improve the firing by the Draper Sight to warrant production. An outgrowth of this project was the development by NDRC of a small computing mechanism and a modification

on the Draper Sight whereby automatic angular tracking by radar of aircraft targets could be used with the Draper Sight to provide blind firing with 40-mm. guns. From the standpoint of technical development, this project, known as Project 151 (assisted by Section T, OSRD), was eminently successful. A favorable report was rendered by the Naval Research Laboratory, but the system was not adopted. Fortunately the Japanese airplane menace under conditions of poor visibility reached disastrous proportions only during the closing phase of the war.

The members of the Radiation Laboratory concerned with these problems and the members of Division 7 of NDRC became convinced that a fully integrated design of a complete radar fire-control system was required. In connection with its successful development of the proximity fuze for antiaircraft shells, Section T of OSRD apparently reached this same conclusion independently at about the same time. The development to meet this need is discussed in a later chapter. The Section T group proposed a solution of a frankly interim character but with emphasis on immediate availability. Radar already in production was to be modified and large portions of the Draper equipment were to be adapted.

The Radiation Laboratory project (in collaboration with Division 7) aimed at a more ambitious but less immediate result. This project was to be carried through to complete engineering, design, the building of manufacturing prototypes, the furnishing of complete manufacturing drawings, and the setting up of a manufacturing source and all necessary vendors. This approach was necessitated by the past experience in which laboratory prototypes had been found wanting, not operationally, but because of the long time element between laboratory research and the subsequent engineering for production. The plan required the following steps (since organizational lines would be crossed): —

1. Complete support from the Chiefs of Divisions 7 and 14 of NDRC.
2. Complete co-operation of the Director of the Radiation Laboratory.
3. The sympathetic understanding of NDRC and the Director of OSRD.
4. The establishment of adequate contracting with a company able to assist in the engineering and also able to manufacture.
5. The sponsorship and co-operation of the Bureau of Ordnance.

The Bureau of Ordnance indicated its support by requesting the establishment of the Project NO-166 in a letter dated May 18, 1943. The backing of Captain Emerson Murphy and particularly that of Captain D. P. Tucker was very heartening. The support given by H. L. Hazen, Chief of Division 7, and by A. L. Loomis, Chief of Division 14, served as an inspiration to the people working on the project. The director of the Radiation Laboratory, L. A. DuBridge, indicated his attitude repeatedly by placing the Gun Fire Control System Mark 56 within the first group of the five most important projects at that Laboratory even though the Joint Radar Development Committee of the Communications Board, Combined Chiefs of Staff, did not so rate it. The General Electric Company was brought into the picture by an NDRC contract, first for the development of a suitable gyro unit and later for supplying engineering, parts, and the construction of two complete director systems.

It would be idle to deny that a considerable jurisdictional conflict arose within OSRD in connection with these activities. There is no point in going into details of the argument in which personalities were involved but some consideration of the underlying causes may be worth while. The initial organization of NDRC had two important features. Great flexibility was permitted as to the mode of operation of its various Divisions, Sections, and even individual projects. Quite strict compartmentalization between Divisions was maintained for security reasons. The first of these policies helped get on with the job by letting each group find its most effective means of operation. The second is a necessary evil in military matters and had to be stressed initially to gain the confidence of the Armed Services. At later stages, when two or more portions of the whole organization became involved in the same problem each of these general policies could give rise to considerable misunderstanding.

Division 14 acted as a board of directors in guiding the radar program and left to its principal contractor, the Radiation Laboratory, great freedom in its choice of projects and in assigning priorities among the projects, as well as most of the detailed liaison with the Services. Division 7 on the other hand exercised close technical as well as administrative supervision over the work of its contractors. When the Radiation Laboratory entered the field of fire control as a consequence of its radar

activities, its customary freedom of activity came into conflict with the detailed supervision of Division 7. The relation with Section T was even more complicated. Its principal agency, the Applied Physics Laboratory of Johns Hopkins University, operated with the same freedom as the Radiation Laboratory although under the immediate direction of the Chief of Section T. By this time Section T had split off from NDRC and reported immediately to the Director of OSRD. Section T activities were financed by funds transferred to OSRD from the Navy. Its policies were determined in conference with representatives of the Bureau of Ordnance. Activities overlapping those of NDRC Divisions sometimes were undertaken without the initial knowledge of the NDRC Division charged with the same responsibility. Security provisions tended to aggravate the misunderstandings that arose in this connection. The problems involved in the relationships between Division 7 and Section T; between Division 7 and the Radiation Laboratory; and between Division 7 and the Bureau of Ordnance ran concurrently. It will suffice to give the end result. It was agreed among Bush, Director of OSRD, Tuve, Chief of Section T, and Hazen, Chief of Division 7, that Section T would undertake a program to provide for the Navy at the earliest practicable date an interim fire-control system for the 5-inch 38-caliber gun;[1] NDRC (Divisions 7 and 14) would undertake the development of a new fully integrated radar fire-control system of an "ultimate" type. It was also agreed between Hazen, Chief of Division 7, and L. A. DuBridge, Director of the Radiation Laboratory, that I. A. Getting would be charged with the direction of the project, both as a Member of Division 7, NDRC, and as head of the Fire Control Division, Division 8 of the Radiation Laboratory. This dual position for Getting insured close co-operation between the Radiation Laboratory and Division 7 of NDRC.

Section 7.6 was formed to supplement this agreement by bringing together civilians from the groups most closely involved in Naval fire control. While the meetings were helpful on an informal basis, the Section did not get to function as effectively as might have been hoped. The war was well into its final stage, but there also were two other contributory reasons: (1) most of the men were already overloaded by prior duties as technical executives of large enterprises; (2) they could

[1] See Chapter XVII.

not separate themselves from their contractual responsibilities to the Bureau of Ordnance. Under the latter, these men were not permitted to discuss technical matters performed under Navy contract or even to discuss, without explicit permission, general plans when items were classified. There seemed to be a third contributing reason; trade-practice secrets among the companies. These conditions were in marked contrast with the radar field, a new field dominated in a large part by the Radiation Laboratory and fully open to all participants on technical and military matters. There is no doubt that, had the war continued, Section 7.6 would have provided for a broader exchange of ideas in the Naval Fire Control field, for the relaxing of unnecessary security restrictions on purely technical matters and for the inclusion of ideas emanating from commercial and other governmental agencies. In addition to the Gun Fire Control System Mark 56 (NO-166), the new Section inherited all projects within the Division which had to do with Naval Fire Control or with radar directly: Project NO-127, the development of a motor-torpedo-boat torpedo director using radar data, and the development of a torpedo director using radar data for destroyers and light cruisers; Project OD-100, the development of a field chronograph using radar. These remained the only projects undertaken by Section 7.6.

All projects taken over by this Section prospered during the rest of the war. The Gun Fire Control System Mark 56 was carried on at the Radiation Laboratory under contract to Division 14 of NDRC, at the General Electric Company under a joint contract of Division 14 and Division 7, and at the Librascope Corporation under contract to Division 7. The technical direction of the project came from the Radiation Laboratory. Just before the end of the war, the Navy placed a production contract with the General Electric Company for a substantial number of units even though full prototypes were not yet available. The torpedo director for motor torpedo boats was tested at Florida and found inadequate generally because of inaccuracies in the existing equipment in motor torpedo boats. A program was set up to correct these deficiencies, but the program was not completed before the end of the war. The program to develop a new director for torpedoes on light cruisers and destroyers was modified to fit the Bureau of Ordnance program. The modified program consisted of changing

the present torpedo director Mark 27 by the addition of suitable retransmission equipment and radar presentation equipment to permit efficient radar firing. This project, at the General Electric Company, was not completed before the end of the war, but was taken over by the Bureau of Ordnance. The development of a field chronograph by the Westinghouse Electric and Manufacturing Company was quite successful. It led to a small production order sponsored by NDRC at the request of the Army and Navy and provided the first real portable chronograph for the Services.

CHAPTER X

EARLY PROXIMITY FUZES

THE MAJOR errors in artillery fire are those in range. This is due to a number of causes, including errors in range estimation, propellant variation, gun erosion, fuze setting (including variation in dead time), and fuze operation. For a few limited applications, contact fuzes eliminate these errors but against aircraft the chance of a direct hit is infinitesimal. Against certain ground targets, greater damage is obtained by exploding the shell in the air. Any device which would allow the time of explosion to be controlled by the presence of the target, rather than by a preset mechanism, would result in an enormous increase in the efficiency of fire. Such is the purpose and importance of proximity fuzes. Similar considerations apply to projectiles like rockets and bombs.

Any statement regarding the early history of the radio proximity fuze must be prefaced by the remark that the broad idea is not assignable to any single individual or any single group. Even its designation as the VT (variable time) fuze seems to have originated independently in Great Britain and in America. The wartime requirements of secrecy and the limited opportunity for the transfer of information confused the picture in its early days. This much is certain: influence fuzes of various types were independently suggested by individuals in America and in Britain (and no doubt in other countries as well) prior to and including 1940. The general ideas embodied in these fuzes were independently invented and reinvented by various individuals, but no practical suggestion for making an operating device was presented. Under the immediate pressure of war, the British appreciated the importance of these devices before American officers and civilians were urgently concerned with such matters.

The operating principles of the photoelectric and radio types of proximity fuzes — each might have a number of embodiments for varied ordnance applications — were relatively easy to demonstrate in laboratory-built models. The major task was to develop production de-

signs of the fuze and of its various components which would be suitable for inexpensive manufacture in astronomical quantities and with the highest standard of reliability. Its accomplishment called for teamwork of a high order between research laboratories, manufacturers, and the Armed Services.

Although at first glance the dollar cost of a weapon in wartime seems relatively unimportant, this cost is really a measure of the manpower and facilities required for the production of the device. These resources were often far scarcer than dollars. Never before had electronic equipment been considered an expendable item to be produced in quantities measured in millions. As one example, 140,000,000 vacuum tubes were produced for fuze use before the end of the war (450,000 tubes a day for shell fuzes only). As the war ended, approximately 25 per cent of the entire electronics industry in the country was devoted to fuze production. The importance of the design of low-cost fuzes and fuze components is clear.

In most previous applications of electronic devices, the failure of a component could be cured by replacement. Improper adjustment could be remedied even in the field. In the proximity-fuze application, testing destroys the samples. Representative samples were diverted from the production lines for tests according to the statistical methods of quality control. This was important, for failure of the fuze to function might present the enemy with an intact sample of the secret weapon. Premature functioning might be dangerous, and certainly would be wasteful.

For one class of fuze applications, tubes of amazing ruggedness were developed. Space requirements were more stringent than those in conventional electronics. As the war progressed, newer applications called for even smaller fuzes. This development of smaller, cheaper, and more rugged components is a major contribution toward increasing postwar applications of electronics.

In the early stages of development of the VT-fuze, the British emphasis, and to a large extent the official NDRC emphasis, was directed toward the development of fuzes for use on bombs and rockets. The initial United States Army interest was almost exclusively in this direction. There was also somewhat less interest in a radio proximity fuze than in the more obvious and simpler photoelectric form. The British

had under construction a simple, low-sensitivity "pulse fuze" for shell use. It was to be triggered from the ground by radio control. The attempt to produce triodes, pentodes, and other complicated electronic items to be sufficiently rugged for shell use was an American idea, initially regarded as naïve by many persons in both countries. Emphasis on a self-contained influence fuze for antiaircraft shells, with a triggering pattern properly related to shell fragmentation, was distinctly an American project.

In early months, solutions were found for the problems which had appeared to make the shell fuze nearly impossible. With each step toward success, enthusiasm grew for the shell-fuze project, and with it, effective backing from the Services. Proximity fuzes would clearly be useful in bombs and rockets. If they were possible in antiaircraft shells, defense against air attack would gain by a strong new order of magnitude. Navy support for shell fuzes was strong and steady. Army interest lay in the bomb and rocket fuzes and, it is only fair to report, was not as strong and not nearly as consistent. Delays were experienced in the associated rocket program. Consequently the shell-fuze project forged ahead more rapidly. Shell fuzes were first used in combat by the U.S.S. *Helena* in the Pacific on January 5, 1943. Half a year later, VT-fuzes began to be supplied to the Royal Navy. Originally the use of the fuzes was restricted to firing over water, where there was no possibility that duds would fall into enemy hands. This restriction was relaxed to allow use against buzz-bombs in England in the summer of 1944. Finally, December 18, 1944, the fuzes were used by the American armies against enemy ground troops in the Battle of the Bulge. Here they helped to stop the last great German drive of the war.

As a consequence of several factors, including less definite objectives and less effective Service support, the proximity fuzes for bombs and rockets were later in reaching combat use. VT-fuzed bombs were first used in February 1945, against Iwo Jima. In the last month of the war, about one third of all the bombs dropped on Japan by carrier planes of the Third Fleet had VT-fuzes. A large production of mortar fuzes had been started, by this time, for use in the invasion of Japan. Because of the sudden end of the war, these mortar fuzes did not reach combat use.

Not all of the delay could be attributed to the causes outlined above.

Unforeseen technical problems arose which were not present with the apparently more difficult case of rotating missiles. The centrifugal forces in rotating missiles caused difficulties in vacuum tube construction, but were very useful in activating the power-supply battery. In the shell fuze, a glass container full of electrolyte is broken by impact when the gun is fired. Centrifugal forces then distribute the fluid between the plates of the battery and whirl off excess fluid which would otherwise cause an internal short circuit. Eventually small wind-driven generators were adopted for use in the nonrotating missiles. This in turn introduced a new series of difficulties because of the mechanical vibrations from the high-speed rotation in the generator.

In the summer of 1940 as NDRC started operations, there was considerable emphasis, as has already been pointed out, on methods of defense against enemy bombers. It was believed, for instance, that formations of bombing planes could themselves be bombed from above, provided some form of influence fuze was available to detonate the bombs as they passed near their flying target. Antiaircraft rockets were also in development at this time and would clearly benefit from the use of a proximity fuze. About two months before direct word of British activities was received in September 1940, C. C. Lauritsen learned that the British were obtaining large quantities of thyratrons and photoelectric cells from the Western Electric Company, and surmised that these were intended for proximity fuzes. This was one of the subjects discussed in the initial planning of Division A. On August 17, 1940, Section T of that Division was established with the directives: (a) the development of "influence" fuzes of any type; and (b) the formulation and exploration of new projects in ordnance. The initial activities of the Section were carried out by M. A. Tuve and his associates, L. R. Hafstad and R. B. Roberts, at the Department of Terrestrial Magnetism of the Carnegie Institution of Washington. The terms of contracts with CIW were unique among NDRC contracts. The Government paid for materials used and for additional persons hired by the Institution to work on the project. CIW received no overhead allowance, and was not reimbursed for the salaries of its regular staff members. This placed CIW on exactly the same basis for compensation as was used when OSRD funds were transferred to a Government agency such as the National Bureau of Standards.

When Tuve started work, he was handicapped by the fact that his colleagues in the laboratory had not yet been cleared to receive secret information. In spite of this, experimental work started immediately to determine the amount of mechanical shock that various types of vacuum tubes could stand. Laboratory tests showed that standard radio tubes were surprisingly rugged and that the hearing-aid type of tube was even better. Shortly thereafter a homemade gun was constructed from a piece of gas pipe. This was set up on a friend's farm at Vienna, Virginia, just outside of Washington and shooting tests were begun.

As Hafstad and Roberts were assigned to the photoelectric-fuze investigations, H. H. Porter was asked in September to join the group to take over the testing of fuze components by shooting, drop tests, and centrifuging. Within a few weeks the photoelectric fuze was ready for fly-over tests at Dahlgren — tests in which the fuze remains suspended near the ground and an airplane flies over it so that the reactions of the fuze to its intended target can be measured. In the winter of 1940–41, the photoelectric bomb fuzes were tested in bomb drops from airplanes. Shortly after the photoelectric fuze tests, radio bomb fuzes were similarly tested. Other possible approaches to the problem had not been overlooked. G. K. Green investigated an acoustic type of bomb fuze, with microphones set to hear only at angles which would produce the proper directional effect. A parallel contract in this field was set up at the Bell Telephone Laboratories. J. A. Bearden and associates began work at the Radiation Laboratory on the Homewood campus of Johns Hopkins University on various aspects of the acoustic fuze, including modification of a British spinning acoustic fuze for bombs.

Electrostatic fields around an airplane were exhaustively investigated by E. J. Workman at the University of New Mexico under one of the Section T-NDRC associated contracts. It was hoped that a fuze could be triggered in the vicinity of a plane by the interaction of the electrostatic fields of plane and shell.

After certain designs of photoelectric and radio fuzes for bombs had been successfully tested at Dahlgren, other possible approaches to this problem were dropped in their favor. This did not necessarily imply that other solutions were not possible, but merely that in time

of threatened war, only those approaches which show promise of rapid application are feasible. The radio fuze appeared the most promising. Especially for shells, the use of a radio-type fuze looked like the best bet since it appeared possible to design special miniature radio tubes which could be reproduced in quantity.'

On September 17, 1940, the Tizard Mission for exchange of scientific information between the United Kingdom and the United States first met with NDRC personnel, at which time British progress on proximity devices was disclosed. It was learned that photoelectric bomb fuzes had made the greatest progress and that radio fuzes for shells were considered only as a vague possibility.

It is of considerable interest to quote from a report from Tuve to Tolman, to review for historical purposes, the progress made by Section T: ". . . most of the effort has been concentrated on five projects under the general heading of Proximity Fuzes: 1) Photo-Electric, 2) Acoustic, 3) Radio (several types), 4) Radio 'noise' from shielded ignition systems, 5) Special radio-type fuze for armor-piercing rocket-bomb. A good beginning has been made on the problem of developing proximity fuzes (or ground-controlled fuzes) for use in anti-aircraft shells, involving the development of rugged electronic devices and rugged batteries to stand the mechanical stress inherent in such an application, the provision of methods and equipment for reproducing these stresses, for tests to guide the development work and an approach toward provision of a system for firing the AA shell at the desired point on its trajectory by an impulse transmitted from the ground. Three other projects have been: very considerable assistance to C. N. Hickman (Section H — Propulsion) in the construction, development and testing of the A-P Rocket-Bomb; brief preliminary study of the possibility of correcting the trajectory of a bomb during flight to increase the probability of striking a desired target; technical study, design and procurement estimates relating to an apparatus which will give the magnetic field distribution at various distances from a de-gaussed ship on the basis of measurements in a single plane, designed to eliminate need for corps of computers on this problem.[1] A separate project covers the design of 'arming devices' required for protection when using the various

[1] (Editor's Note: The origin of this project and further work on it are described in the concluding paragraphs of Chapter XX, page 185.)

fuzes above mentioned. Thought and attention has been given to other items not above listed."

In November 1940, the National Bureau of Standards was brought into the picture as an additional agency to work on the problems of Division A. Alexander Ellett of the State University of Iowa came to Washington and spent several weeks at the Department of Terrestrial Magnetism. He then moved to the Bureau of Standards, where he took charge of the work being done there for Division A, most of which was on proximity fuzes. Formally, the work was channeled through a new Section "E" of Division A, with Ellett as Chairman, and NDRC funds were transferred for the purpose.

During the first half of 1941, parallel work was done at the two laboratories on the development of the radio form of proximity fuze. There was close co-operation between the groups at this time with interchange of ideas and personnel. When, somewhat later (July 1941) the Navy insisted that the radio shell fuze be given high priority, the work was regrouped. The Department of Terrestrial Magnetism and the other Section T contractors went ahead exclusively on the shell fuze. The National Bureau of Standards and the contractors of Section E took over the whole program of fuzes for bombs and rockets. Security restrictions increasingly limited the exchange of ideas between the two large groups. This narrative will now follow the activities of Section T on the development of shell fuzes and on associated projects. In Chapter XX, it will return to Section E and its work.

CHAPTER XI

RADIO STATIONS FOR SHELLS

RADIO oscillators had frequently been sent aloft in balloons to take soundings in the upper atmosphere, but to fire one from a gun was a totally new departure. After some of the problems were under way, an oscillator was fired from a 37-mm. howitzer early in 1941 at Vienna, Virginia, and listened to by radio during full flight. This was essentially a dress rehearsal for a test at the Naval Proving Ground at Dahlgren in which the first oscillators were to be fired in standard Navy 5-inch shells. E. J. Workman came to Washington after the completion of some New Mexico tests on electrostatic fields about aircraft to work with Tuve, Roberts, and H. R. Crane on radio shell fuzes. These men mounted experimental miniature triodes as oscillators in howitzer shells, powered by "Minimax" type of battery made by the National Carbon Company, and fired each in turn, listening hopefully. Only one was heard, but this one continued to radiate signals even after it had landed! This phenomenon started a train of arguments as to whether it really had been heard. Could an oscillator lying on the ground be heard? Would it continue to oscillate as it approached the ground? Identical oscillators were dropped from balloons and listened for; they were heard even at the time of impact, which apparently made no appreciable difference, and afterward while lying on the ground, which some had argued would not happen. Some thought that the landing would surely be discernible by the radiated signal. Much ado about nothing perhaps, but at this stage of the game it was important to establish that an oscillator had really been heard in flight!

A short time after the Vienna test two more oscillators, constructed by H. R. Crane and R. B. Brode, were heard in full flight over the water at Dahlgren. A party of interested observers — or listeners — including Tolman, Lauritsen, Tuve, and Roberts were seated in a boat beneath the trajectory, earphones pressed to their ears, and heard the first radio signal ever sent from a spinning 5-inch shell. No signal from Mars could have caused greater joy and exuberance!

These experiments opened a new era in ordnance and ballistics. They were all that the late Captain S. A. Shumaker (Navy BuOrd) needed to set an unequivocal first priority on the radio shell fuze and clear the way for this radical innovation in ordnance.

The groups, which produced successful oscillators almost simultaneously, were in fact working on two fundamental approaches to the VT-fuze—these might well be called the remote-control or ground-control fuze and the "ripple" fuze, which is automatic. In the remote-control version on which Crane and his associates were at work, the shell carried only a receiver, the triggering signal being sent from the ground. It had obvious advantages over the time fuze, since control over burst time could be exercised even after the shell had left the gun and computations could be continued, so to speak, until the instant before detonation. A computing device which predicted the time of closest approach of target and shell, the so-called "slant range indicator," was developed for use with the pulse fuze by the Johns Hopkins Radiation Laboratory under the direction of J. A. Bearden.

At the time of the first successful oscillator test in a 5-inch shell, the pulse-fuze project was getting well underway. Analysis of relative damage probabilities of pulse fuze versus the automatic radio fuze, which carried its own transmitter as well as receiver, led to the eventual abandonment of the former project. This was carried up to the point where it was reasonably certain that the other type could be made to operate successfully. Even if the pulse fuze could have operated perfectly, the difficulties of triggering the correct shell out of the thousands of shells in the air during a Fleet engagement would have been enormous. In the ripple or automatic radio fuze, the projectile carries its own transmitter as well as receiver and is automatically triggered when the intensity of the signal, reflected from a near-by target, becomes critically great.

The VT-fuze has a great advantage over the remote-control, or pulse, type of fuze in that no ballistic computations of any sort are necessary for fuze setting—simple proximity to target being the sole requirement. It is also far more useful over land, as in field-artillery howitzer use, in which the automatic feature is particularly important for targets obscured by terrain or atmospheric conditions. It is the ideal fuze for air-burst artillery fire.

The first successful oscillator was made possible by two major factors — a sufficiently rugged tube and a rugged power supply or battery. The development of the rugged tube, without which VT-fuzes would have been totally impossible, is so significant a chapter in the history of the VT-fuze that it has been treated separately.

The development of the energizer or battery is also an integral part of the story, not only because of the ingenuity displayed in laboratory development but because it is an outstanding example of the solid support American industry gave to national defense.

From the time that M. A. Tuve first visited the offices of the National Carbon Company in Cleveland in the fall of 1940 to discuss the need for compact, rugged batteries, to the end of the war, this company worked in close co-operation with Section T, pioneering in the development of new versions of dry batteries and in the ingenious and efficient reserve type of battery (page 148), a program involving the over-all production of nearly 30,000,000 batteries.

In the fall of 1940, Section T was primarily concerned with photoelectric and radio fuzes for bombs and rockets which, of course, must also have a supply of power. Tuve requested that the National Carbon Company consider the production of several types of small dry batteries, the smallest of which should be one inch in diameter and two inches high, another two inches by three inches. With the ultimate shell-fuze objective in mind, it was specified that all of these batteries must withstand 20,000g and high axial spins. One week after this request, the first eleven battery units had been shipped for test at the Vienna farm. The National Carbon Company at this time also agreed not to furnish small batteries to anyone else; this they did from a purely security standpoint, so that the very idea of ultra-small batteries would not circulate.

It was soon realized that 1-inch diameter dry batteries were too small to be feasible. Two larger types, the 2-inch and the 1½-inch, were put into pilot production, the former going into active production in January 1942. This 2-inch dry battery was the standard power supply for the United States Navy VT-fuzes until replaced by the 2-inch reserve battery.

As is related in greater detail elsewhere the smaller 1½-inch dry battery proved to have an unstable shelf-life. Before reaching an extensive

production stage, it was replaced by the reserve or wet battery which was first developed especially for use with VT-fuzes.

The demonstration that oscillators in shells could be heard in full flight cleared the way for the next major step toward a radio proximity fuze — the amplification of the small electrical variations which must be present in the circuit of the tiny radio transmitter when it passes near a reflecting object, in order to use this amplified signal to trigger the explosive charge.

It was not certain that this could be done. Three more tubes (two amplifiers and a thyratron) and associated circuits, properly coupled to the oscillator, were involved. Could the desired signal be amplified without amplifying the tube noises (microphonics) and other electrical noises resulting from severe vibrations set up in the shell under the stress of gunfire? Simplest investigations showed that any vibration of the tube elements, for example, would introduce spurious signals which might easily be several orders of magnitude greater than the feeble target signal; any of these could cause premature action of the fuze. "Microphonics" was a particularly sinister specter and led some visiting scientific consultants to state unequivocally that VT-fuzes for shells were an *a priori* impossibility.

Field experiments seemingly verified these predictions. Tests resulted in a long and discouraging series of malfunctioning units. Prematures, prematures, prematures! But some complete fuzes worked. In a memo to Lauritsen Tuve wrote: "It is my pleasant duty to inform you that we have evidently been successful in operating a radio-pulse fuze from the ground. The experiment must be repeated, of course, but it is desirable to let you know of the present situation. This first evidently successful attempt to pulse a radio-fuze in a projectile from the ground occurred in connection with some tests of the radio-ripple circuits which have been designed and constructed."

These successful units were at first so rare as to be "sports" but a VT shell fuze had been achieved — a laboratory model in every sense of the word. The early tubes were handmade in small experimental batches by Raytheon and Hytron engineers; batteries were also experimental, scaled down from the Minimax type provided by the National Carbon Company. Condensers (by the Solar Company) were of experimental manufacture since all available commercial sizes were far

too large for use in the small fuze design. Of all the technical and engineering efforts involved in creating the VT-fuze, the development of rugged miniature tubes stands out in bold relief. In the VT-fuze, the rugged tube plays the key and critical role; without it the VT shell fuze would have remained an inventor's dream. Without the promise of success given by the early drop tests and homemade gun firings, the other phases of the program would probably never have been initiated. The VT-fuze depends on the ability to manufacture in mass production a tube whose numerous parts (there are more than forty individual parts in each tube) will remain mechanically stable and maintain their electrical functions under forces which are outside the range of everyday usage.

The magnitude of the difficulties at the start of the tube project can perhaps best be realized when it is remembered that ordinary tubes are fragile; in the routine course of handling and shipping, several hundred thousand dollars' worth of standard radio tubes were ruined each year.

It was with considerable trepidation that R. B. Roberts, H. H. Porter, and D. P. Mitchell approached the first tests of small standard commercial tubes to determine the limiting accelerations these tubes would stand and to determine which parts were most susceptible to injury. The first tests were indeed elementary—tubes embedded in wax and mounted in lead hemispheres were dropped from the roof of a DTM laboratory building, the degree of flattening of the lead hemispheres indicating the order of shock experienced by the tube. Raytheon and Hytron hearing-aid tubes were tested at the Vienna farm and these companies were invited to contribute in modifying the structure of these tubes. L. K. Marshall and his Raytheon engineers were particularly helpful. From the start, tubes were "potted" in Cerese wax, first suggested by Dana Mitchell, and no superior material was found during the entire course of the project. It is most exceptional in experimental techniques that a first guess should prove superior to hundreds of other materials tried later.

All tests proved the urgent need for fundamental revision of tube mechanical design. In some of the early tests, the elements ended up as a little ball at the bottom of the tubes. A major share of the credit for the final mechanical design of the rugged tube must go to R. D. Mindlin,

as it must to L. G. Hector for the electrical characteristics of these tubes. When Mitchell first approached Mindlin on the problem of rugged tubes, security restrictions were extremely severe, so Mindlin was asked whether he could design a tube for meteorological sounding balloons capable of withstanding a fall to earth. A short while later when it was further specified that these tubes, in addition to this considerable fall, must be able to withstand spins of four or five hundred revolutions a second, Mindlin guessed something of their intended use. He then computed curves showing at what accelerations various materials, structural forms, and connections would fail. Experiment substantiated to a remarkably high degree the accuracy of these computations, and they were used as a basis of design for all rugged tubes—truly a proof to disbelievers in the validity of theoretical stress-and-strain analysis. At about this time contracts were let with Hytron and Raytheon for the experimental production of rugged tubes by modifying existing types. The Bell Telephone Laboratories in an early contract were asked to approach the tube problem independently.

In addition to firings in the homemade gun, and later in a 37-mm. howitzer furnished by the Navy, much tube-testing work was done in special centrifuges designed by J. W. Beams, University of Virginia. Mitchell recalls that the first few centrifuges were X-rayed by rushing them to a local hospital where they were handled in the same manner as a patient. Rugged laboratory tubes (and the word "laboratory" is highly significant) were obtained eventually, and justified the Section T feeling that a shell fuze was possible. But it is a far cry from a few tubes painstakingly hand-produced under direct supervision of a design expert, to hundreds of thousands of tubes made by mass-production-line methods with unskilled and fluctuating personnel.

It was at this point that the Hygrade-Sylvania Corporation (later Sylvania Electric Products, Inc.) was visited by Hector and Mindlin with a contract amounting to the magnificent total of $250. Sylvania was at this time regarded as merely another possible manufacturer. Roger Wise (then Chief Engineer and later Vice-President in charge of Engineering) had the imagination and foresight to persuade his Board to commit company funds to this project, and to proceed as though commercially manufactured, tiny rugged tubes were a routine matter. Highly significant was the fact that, under Hector's guidance

from the earliest stages, the Sylvania tubes, incorporating the experience gained in the previous testing program of Section T, were designed for production. The factor of the production line was everpresent on the drawing board. Even the farsighted Wise, however, did not visualize the ultimate production rate; he had convinced his somewhat reluctant Board to take the gamble on his word: "It is even possible that we may some day make fifty thousand (tubes) a day." Had he added another zero to his estimate, he would have been closer to the truth, but the reaction of his Board to such a proposal in this case can well be left to speculation!

Designing for production paid off. The fundamental designs for the three basic tubes, the triode, pentode, and thyratron, carried through without major change to the end of the program. They are three-eighths of an inch in diameter and one-and-one-quarter inches long. Their cost decreased from $5.50 initially to about 40¢ at the height of the production program. This decrease resulted not only from mass procurement of materials but from decreased labor costs — the skill of the worker (90 per cent of personnel on tube production lines were women) increased markedly after she overcame her fear of tiny parts. As her fingers became accustomed to her particular intricate task and overcame the psychological barrier of the extremely delicate nature of the operation, factory rates soared. Vertical responsibility in production lines was employed, and teams vied with each other for hour-to-hour and day-to-day scores. In all, approximately 140,000,000 tubes were produced — 56,000,000 in the last year of production. Twenty-three Sylvania plants in all, scattered in Pennsylvania, Ohio, Kentucky, and West Virginia, many of them improvised for the purpose, were used in parts' manufacture and final assembly.

Tube production was almost always ahead of schedule, and quality in general was so good that the rugged tube, once regarded as the great bugaboo of the VT-fuze, was soon taken for granted. There were of course epidemics of failures. In February 1942, when mass production was just getting underway, glass failures became so severe that for a time it was felt the only solution would be the use of all-metal tubes. Again, from time to time, waves of filament failures occurred, always clearing up as mysteriously as they appeared despite frantic, detailed, and intensive laboratory and theoretical investigations. A large part of

peak loads consisted of "tube shoots." Tube production samples, mounted in blocks of eight, were fired and recovered. In this way a close check was kept on tube-manufacturing trends, one part in a large interlocking system of quality control which was a prime responsibility of the Section throughout the VT-fuze program.

The art of making rugged tubes is the result of the sum total of many factors. High in importance on the list is filament suspension. One-third the diameter of a human hair, the filament must be centered under tension to insure that no objectionable microphonics will be produced. The technique of suspension to maintain this tension in flight after gunfire, is, as can be imagined, a major accomplishment. This problem alone led Hector to state early in the rugged-tube program: "Success looked like a thousand-to-one shot." The miniature rugged tube is now a standard equipment; its performance is proved, and the techniques of its manufacture will presumably have many postwar applications.

CHAPTER XII

PRODUCTION PROBLEMS

THOUGH THE war had not yet reached the United States, the Navy was urgent in its drive for a VT-fuze; one Navy official stressed that each day's delay meant the potential loss of one hundred and fifty lives if war should come. To advance the project in every possible way, the Navy Bureau of Ordnance placed a separate development contract with the Crosley Corporation in November 1941 which, though specifying technical liaison with Section T, was independent of NDRC administration. This arrangement was made with the particular plan in view of having a large industrial company with considerable electronic manufacturing experience ready to launch into full-scale production of fuzes at the earliest possible moment.

Captain Shumaker had defined that moment to be when pilot-line fuzes returned at least a 50 per cent operability score in official proving-ground tests. This might seem to be an amazingly lenient stipulation; it was made, however, with full recognition that even a 50 per cent operable VT-fuze was more lethal than the existing time fuzes by virtue of the estimated relative advantages.

A pilot line was established at Crosley and work started with enthusiasm. It is no reflection on the ability of the Crosley engineers that this early independent development program failed to produce workable fuzes. This is rather an acknowledgment of the inherent difficulties encountered in trying to manufacture a new and complicated development device for the first time. At a somewhat later stage the development work at Crosley was, by Navy request, more closely integrated with that carried on at Section T. This made possible the rapid interchange of technical information between laboratory and factory and resulted in Crosley's becoming the first industrial producer of VT-fuzes. It is greatly to the credit of the Crosley engineers, whose experience in designing for production was invaluable, that many of the design changes incorporated in the later models of the fuze were suggested originally or independently by the Crosley staff.

During the fall of 1941 and early 1942, Section T fought the battle of prematures and broken tubes. The Hytron, Raytheon, Bell Telephone Laboratories, and more recently the Sylvania companies were each supplying experimental batches of rugged tubes which were then tested in full circuits, or in inert shells to check mechanical failures. In most of the active units, oscillators could be heard throughout most of the flight, but the use of three additional tubes in the amplifier greatly increased the chances of unit failure at this particular stage when each rugged tube was merely on the threshold of ruggedness. When the tubes did not fail the majority of units prematured. Battery noises and microphonics were apparently the prime difficulties here. The study of microphonics particularly was given great attention at this time and throughout the course of the work, resulting perhaps in the most exhaustive work ever done on tube microphonics. Extensive laboratory studies were made, and in the field, special radiosondes modulated by amplifier-tube microphonics and recorded by oscillograph techniques at the listening station presented a graphic account of microphonics in flight. These investigations were particularly helpful in determining microphonic frequencies.

Many times during this critical period, success seemed just within reach. Field tests continued, however, to produce scores — "seven fired, four prematured, two pulsed and one dud." Matters got worse before they got better. Tuve declared a complete emergency on rugged tubes and diverted all Section T technical manpower to the solution of this problem in co-operation with the tube company engineers. This was an oft-repeated story; whenever increased quantities of rugged tubes were needed, tube quality invariably went down, re-emphasizing the salient problem of VT shell fuzes: individual rugged tubes or fuzes could be made in the laboratory and on small pilot lines but when quantity was increased, quality slumped.

Yet in that same month, some of the Section T men were optimistic enough to try the first test in 5-inch guns at Dahlgren. Ten units were fired, six prematured, and four wouldn't work — so it was back to the laboratory again. The superiority of recovery testing was highlighted by field tests — there was no other way of knowing the cause of failures; only the painstaking recovery-postmortem technique could reveal why units failed.

Every technical trick was tried to improve the mechanical stability of fuze components. Here the contributions of the "radio hams" in the teams were especially valuable, since these men were quite accustomed to improvising and tinkering. There was no time for following accepted scientific methods in which all variables save one are kept constant and each full effect determined, step by step. Even had there been time, this would not have been feasible since test conditions varied, particularly regarding wear of the different gun barrels. The "best bet" method had to be employed. Each new experimental fuze model incorporated a host of ideas which might conceivably make the device work properly. The list of components of each new model sent to the test field read like a doctor's prescription — in fact, they were called prescriptions for detailing the particular types and values of tubes, condensers, resistors, etc., by minor details of construction. The primary purpose was to be able to place the identical model into production should success come.

Prescriptions officially came into use when tests were made at Dahlgren and Stump Neck, in order that the same material would be given both types of tests. Prescription A was fired on January 29, 1942, and returned a score of 52 per cent operability. This, at long last, seemed to be IT. Captain Shumaker's goal for starting production had been reached, and Crosley was asked to go into pilot production with full-scale production to occur as rapidly as possible. Prescription followed prescription in the constant attempt to squeeze out higher test scores. Better-designed units resulted in higher consistency in performance but it was a long while before scores improved materially — in fact, not until fuzes were flowing smoothly from production lines.

Prescriptions for VT-fuzes followed each other in rapid succession. Translating laboratory successes into consistently performing, factory-produced equipment often appeared impossible. As early as July, 1942, the first factory-produced material scored as high as 70 per cent. Good VT-fuzes were possible; the other ingredient in the prescription for success was patience in the step-by-step adaptation of the fuze design to factory production.

Manufactured at Erwood, one of the prescriptions, Prescription F, was the first to be fired successfully against a bona fide service target. Even successful operation at the Dahlgren Proving Ground did not

necessarily mean an effective battle device. A successful proximity fuze *must* detonate at such a point in its trajectory that shell fragments will hit the target. Since a shell burst is not spherically symmetrical, the fragments being concentrated in a fairly well defined "cone," the shell must burst when the target is within the volume of the cone. A burst occurring a little too early or a little too late will spray fragments harmlessly. A proximity fuze which bursts systematically too early or too late is, of course, totally useless, and a time fuze would be infinitely preferable in this case. This point cannot be overstressed. It is the reason that Captain Schuyler at first termed an improperly designed proximity fuze as "the world's most complicated form of self-destroying ammunition."

Only a test against a bona fide target could satisfactorily "prove-in" the VT-fuze. The first test of this kind took place in the spring of 1942, at the Parris Island Marine Base. A Taylor Cub plane, suspended from a Navy kite balloon about 600 feet below the balloon and 900 feet above the water, served as the first VT shell fuze target. The gun, a 5"/50, was at a range of 11,000 feet. Of the 182 projectiles fired, 20 per cent were target rippled — a remarkably fine score for the immature state of the art.

Similar tests were conducted later with the first Navy production material (Mark 32) at New River, North Carolina, in December 1942, to obtain a firsthand check on the performance to be expected of the material then in transit to the Pacific. Less than a month later, the Mark 32 brought down its first Jap plane, fulfilling the indications of the New River tests. An observer watching such a test for the first time could not help but feel a spine-tingling thrill; until the newness wore away, he remained entranced with the fact of a technical dream come true — a tiny radio, fired from a gun, speeding along ready to send the firing signal to the explosive train at just the correct instant. When the fact of a technical challenge successfully met was coupled with the fact of a secret and potent answer to the bold threat of enemy aircraft, the sense of having participated significantly in the war effort as part of a team was warmly gratifying.

Gratifying as the Parris Island tests had been, the VT-fuze had yet to experience simulated battle conditions of use in high-explosive shells against moving targets at sea.

This occurred on August 12, 1942, aboard the cruiser *Cleveland* maneuvering in Chesapeake Bay. In these tests, under the technical direction of R. B. Brode, radio-controlled robot planes, or drones, "attacked" the *Cleveland* on typical torpedo-bomber courses. It is interesting to note that just two years later the VT-fuze was to be employed against another sort of robot plane, the deadly V-1 buzz-bomb. Both robot attackers met a new, formidable, and secret opponent.

The Chesapeake tests, as they have since been termed, were spectacularly successful. The tests had been scheduled to last several days, but since all the drones assigned to the tests were brought down in flames, often after only six or eight rounds, the game had to be called because of "no more drones." Erwood- and Sylvania-manufactured material made an especially fine showing in this respect. It is reported that considerable concern was expressed in high, but uninformed Navy circles (VT secrecy extended to the top in all but a few instances) at the unprecedented expenditure of drones, and an official explanation was demanded! It is also related that at considerably lower echelons, among the ships' crew, bets at high odds, placed by an informed few on heretofore improbable shooting scores, paid off! These winnings could not be spent in port, however; shore leaves were canceled — the crew knew too much!

President Roosevelt was one of the few to see the movies that had been taken of the Chesapeake tests. On New Year's Day, 1943, Tuve, Hafstad, and Rear Admiral Parsons (then Commander) accompanied Admiral Blandy and Captain Shumaker to the White House to show the pictures and discuss the progress of the VT-fuze program, of which the President was fully cognizant. This was his first personal contact with the work and with the men who were carrying the top responsibility.

The Chesapeake tests had a galvanizing effect at the Bureau of Ordnance. Whatever skepticism about the VT-fuze had existed in some circles was now swept away, and the cry was for immediate production. Though the Crosley company was still experiencing difficulties in transforming the laboratory model into a mass-production unit, its facilities for production were so good that a Steering Committee of laboratory staff members, headed by Brode, was formed to attack the mass-production problem. In a frenzied few weeks, fifty-nine analytical fir-

ing tests were run at Dahlgren and Newtown Neck. The recovered units from the latter tests were subjected to exhaustive postmortem examinations; manufacturing techniques were improved, and the production problem was licked. The secret was simply that each assembly-line fuze had to be regarded as a custom-built unit. Seemingly minor items which even an experienced production engineer might brush aside as inconsequential might make the difference between a premature and an on-target detonation. A trivial point in static assembly could be transformed by gunfire into a major dynamic factor — a tube placed a fraction of an inch farther in or out of its rubber sock could mean a snapped lead or a broken filament at 20,000 times the acceleration of gravity.

Soon the first ready-for-action Mark 32 fuzes began coming off the Crosley production line. Large compared with the later, compact fuze (Mark 53) which combined the oscillator and amplifier into one nose-encased unit and dispensed with the large mechanical clockwork safety device, the Mark 32 was the only VT-fuze for the Navy's principal large-caliber antiaircraft guns for nearly two years. Fuzes for these guns were manufactured solely by the Crosley and Sylvania companies of Cincinnati, Ohio, and Ipswich, Massachusetts, respectively. The Sylvania fuze production was entirely independent of the Sylvania tube manufacture which was centered at Emporium and Mill Hall, Pennsylvania.

The Bureau of Ordnance let the first regular Navy production contract in February 1942 before design of VT-fuze had been frozen. As a matter of fact, the VT program for three years was a series of technical crises — tube failures, difficulty with reserve batteries, substitution of materials, with the Navy continually pushing for higher production rates. There was a constant fight against shortages of parts and labor, accompanied by changes in military requirements as war progressed. In October 1942, with a test score of 64 per cent on production material, the first frozen design was made for Navy Production on Section T specifications.

Tubes, batteries, safety devices, outer casings, and a host of individual resistors and condensers were received by these and other assemblers of VT-fuzes as Government-furnished material. Close inspection was exercised over all these parts by the Navy, and Section T also maintained a full-scale quality control laboratory. Each assembler, however, per-

formed additional parts-acceptance testing. At times very rough-and-ready testing procedures were used. At one Company, for example, a new type of plastic material was once tested by being tossed against the radiator. If the plastic still resisted this treatment, it was considered rugged enough to fire from a gun.

With so many parts furnished, one might again ask what difficulties could there be in routine assembly. And again, 20,000g is the answer. "Routine assembly" was intricate; assembly of certain fuze models never attained acceptable Dahlgren performance regardless of assembler. Other models proved acceptable from one assembler, while "identical" models from another assembler failed miserably in tests. Each fuze assembler, throughout the war, had his periods of success and of failure — often scores hovered dangerously close to the rejection limit.

Variable factors in fuze manufacture were legion — the answer to failure was "produce more and more, and keep on testing." The technical team, as well as the manufacturers, learned by doing. Sometimes recovery tests and postmortems revealed causes of failures but in some cases they did not. Premature action, the curse of the early VT-fuzes, could seemingly occur spontaneously, like nuclear disintegration! Only after a great deal of practical manufacturing experience had been achieved did premature action slowly decrease, until in late models it had been reduced to a negligible point.

Acutely aware that any new secret weapon, particularly a novel, electronic "magic" device must be psychologically as well as tactically acceptable to the military to insure its full strategic use, Tuve demanded from the start that safety in handling and in firing of the VT-fuze be of an extremely high order. Muzzle bursts, especially in the infancy of the fuze, would have "queered" it for early Service acceptance, and even if eventually made entirely safe, would constitute a strong damping factor on its use as a standard weapon. Special safety devices were, therefore, designed for the VT-fuze; used in addition to the standard safety features of the conventional time and contact fuzes, they were incorporated into the rear fitting of the fuze for pre-fire handling safety and for post-fire arming delay.

The over-all safety margin of the fuze was generally reckoned as one muzzle burst in a million rounds, even though during its entire service history no muzzle burst occurred which could be definitely

traced to a faulty VT-fuze. This makes it the safest artillery fuze ever devised. Six independent safety features made this extremely high record possible; two of these devices, whose sole function was safety, a mercury switch and a reed-spin switch, were, however, the primary safety devices peculiar to the improved models of the VT-fuze.

Reading from top to bottom, an electronic safety feature was incorporated directly in the fuze circuit by which the firing condenser was charged through a resistor, the accumulation of the firing charge thus being delayed by several tenths of a second. Next, the power supply, or reserve battery, constituted a safety device, as proper distribution of the electrolyte could not take place until the shell was in full spin for a fraction of a second. The rear fitting, just below the battery, contained the ingenious mercury and reed-spin switches; and finally the Mark 44 auxiliary detonator, designed by Section T for the fuze, by now in general use. The Mark 44 contains two misaligned explosive charges which are thrown into alignment by the centrifugal force of spin after the release of special detents which prevent any movement before firing.

The rear fitting of the VT-fuze, whose sole function is safety, first incorporated an escapement assembly and a portion of the gear train of a standard, centrifugally-driven clockwork time fuze. Housed with it were the squib and the firing condenser. This was designated as the Rear Fitting Mark 1 and was manufactured in large quantities by the Hoover Company at North Canton, Ohio. In principle, the centrifugally-driven clockwork, after the release of appropriate detents, operated after a delay of the order of one-half a second to open a metal gate across the explosive train passage to the auxiliary detonator. The movement of the gate, in addition, broke a squib-shorting wire. Similar clockwork mechanisms were developed for a smaller British fuze (the 1½-inch Mark 33).

The rapid progress of development toward more compact fuzes for use in smaller shells, and to allow more space for high explosive in larger shells, made it necessary to develop entirely new and smaller types of safety devices. This rear fitting (Mark 8) was only three-quarters of an inch long as contrasted to the 2½-inch Mark 1 and effected, therefore, a very considerable space saving. The Mark 8 rear fitting contained two mercury switches and one reed-spin switch, and replaced the Mark 1 as the standard safety device for the Navy 5-inch

VT-fuzes. These tiny switches, each not much larger than an eraser on the end of a pencil, were simple but ingenious devices which served to make the fuze safe in rough handling, to prevent arming for an appropriate time after firing, and in certain antiaircraft fuzes, to self-destroy the projectile in the event it was not detonated by its intended target.

The mercury switch is simple in conception. It consists of two small chambers separated by a porous barrier (originally chemical filter paper and later sintered nickel); a pellet of mercury in the first or contact chamber provides a low-resistance, electrical short for the electric detonator which initiates the explosive train. Unshorting occurs when the mercury is centrifugally driven through the barrier into the second chamber, or sump. The mass production of these switches, of which 41,000,000 were produced, proved a major problem. Depending on the projectile, various delays from two-tenths of a second to several seconds were required with, however, exceedingly close tolerances for any given application. The time of delay is determined by the porosity of the diaphragm and the spin of the projectile. In the first design which was done by the Geophysical Laboratory of the Carter Oil Company, Tulsa, Oklahoma, special grades of blotting and filter paper were used to control this porosity.

Serious difficulties were encountered for lower-spin projectiles, such as howitzer shells, and a new type of mercury switch designed jointly by the Jefferson Electric Company of Bellwood, Illinois, and the Section T Applied Physics Laboratory solved the low-spin problem. In it, the contact chamber was fabricated of powdered nickel, compressed and sintered, which also acted as the porous diaphragm. Special techniques were devised for manufacturing and testing these sintered metal cups. Pioneer work was done by the Keystone Carbon Company in St. Marys, Pennsylvania, and the Powdered Metal Products Company in Chicago, Illinois. The method of manufacture of nickel cups determined the delay times; even though made in very large quantities, means were developed for extremely close control of mercury flow, probably representing the highest degree of control yet achieved in the field of powder metallurgy.

All told, fourteen models of these switches were manufactured, each having a characteristic delay time. Obviously, high purity of mercury

was demanded and required triple distillation at the factory of the best obtainable mercury. Manufacture of the completed switches was done by the Jefferson Electric Company, and the Hoover Company of North Canton, Ohio. Illustrative of the many techniques developed to control tolerances and quality was the treatment given each of the 100,000 or so switches manufactured daily. It was required that each switch be spun and its delay time measured, with the rejection of switches outside tolerances. All acceptable switches were then spun in reverse to restore the mercury from the sump to its original position in the contact cup; an X-ray analysis then eliminated any switch in which the mercury had failed to spin back. Next, each group of switches undergoing test were "bounced" to simulate rough handling and X-rayed once again to note whether any mercury had been dislodged. As so often happened in a war program, practices were tolerated which would be frowned upon in peacetime. Rejection rates ran extremely high; at one time, four switches had to be rejected for each one accepted, since the Navy would not compromise the safety of gun crews merely in the interest of saving money and time.

A second safety switch in the rear fitting was simplicity itself, but again was subject to a great many manufacturing difficulties. It consisted of a leaf spring, or flexible metal reed, molded at one end in a plastic stopper inserted into an insulated metal cylinder. The reed switch is closed except when held open by centrifugal force. Before firing, the switch serves to short out the firing condenser, thus obviating any remote chance that the firing condenser might accumulate sufficient charge to fire the auxiliary detonator before the proper time. The tension of the spring is adjusted so that return contact will be made as the spin of the shell slows down, and self-destruction can thus be accomplished at any desired portion of the trajectory. If only the pre-fire safety feature is required, as in the case of howitzer fuzes, the switch is adjusted to remain open throughout the entire trajectory. The self-destruction feature was particularly important in certain antiaircraft applications, as for instance during the buzz-bomb campaign, where self-destruction prevented undetonated shells from falling into friendly territory.

The value of being able to recover a unit after it has been fired from a gun is obvious. By subjecting the recovered units to an autopsy, causes

Applied Physics Laboratory

Vertical firing of VT-fuzed shells to determine breakage of components

Experimental range in New Mexico where VT-fuzed shells were fired against suspended dummy aircraft

of failure or malfunctioning can be discovered which would otherwise remain hidden. Indeed, in laboratory parlance, this sort of examination was called a postmortem.

The beginning of recovery testing dates back to the earliest days of Section T investigations. Various parts of development fuzes were dropped from the roof of the Cyclotron Building at the Department of Terrestrial Magnetism, and then examined for damage. After a short interval at the Vienna farm, actual test firing was carried on at Stump Neck on the Potomac, which was an abandoned Navy field. These facilities were quite primitive, consisting of one 57-mm. gun and a field crew described in one early report as "a gunner, a man to load, the man initiating the test, and whoever he could find to help." The shelters were inadequate for storm and shell alike. As the testing load increased, firing had to be carried on under all weather conditions. High winds often made recovery difficult — at one time the winds were so high that the gun had to be tilted 10° from the vertical to bring the shells down into the recovery area. When the shooting was over, it was discovered that the ballistic limits for base-down shells had been exceeded. The shells had all landed nose first, demolishing the units!

The technique of vertical firing was adopted in order to have the spinning shell return to earth base down, so that the components of the fuze would experience the same type of acceleration which would occur in artillery fire. A simple "steel ball on lead plate" gauge was devised to compare the acceleration on landing with that on firing — if the landing g (force of gravity) was greater, that unit was eliminated.

This type of recovery testing requires ground of a particular consistency. Ground that is too soft makes recovery of units almost impossible, while ground that is too hard results in excessive shock in landing. A depth of three feet gives the proper g equivalent. When the facilities at Stump Neck became inadequate for the requirements of the Section T test program, a new location was sought by the Applied Physics Laboratory. T. C. Roberts, in charge of test-field facilities, posed as a farmer interested in growing potatoes, and equipped with maps and a posthole digger, he roamed the eastern seaboard in the vicinity of Washington, looking for a sandy spot with a low water table, and not too many near-by neighbors. Ironically enough, the most favorable locations all seemed to be situated along regular airline routes.

Newtown Neck in St. Mary's County, Maryland, was finally selected, and was made available to the Laboratory through the kind offices of Father Phillips of the Corporation of Roman Catholic Clergymen. Four separate fields were established so that firing and recovery could be carried on simultaneously. At one period during the height of the program, firing was continuous, day and night, for more than a week. In one day, 785 rounds were fired and 770 units were recovered! Farmers in the vicinity complained bitterly that their hens were so disturbed by the continuous noise that they would not lay.

At Stump Neck, the initial procedures were established. As each round was fired, almost vertically into the air, its position of fall was noted by primitive triangulation. Then all hands took posthole diggers and extricated the shells which had buried themselves in the sandy soil. These were conveyed back to the laboratory, postmortemed, and the sometimes bitter results of experience incorporated into the next batch of development units. Several rangers (inert shells) were fired at the beginning of each day's firing to test the winds aloft which determined the drift of the projectiles. With the gun adjusted so that the shells would fall within a relatively narrow field (the target half-circle was about 300 feet in diameter), firing began.

As each unit was fired into the air, a radio operator listened to the oscillator throughout flight to check its functioning, and as the shell approached the end of its trajectory, it was pulsed from an auxiliary transmitter tuned to its frequency. This procedure should not be confused with that used for the "pulse" or remote-control fuze, which contains a receiver only and hence can operate on proximity to target only by means of a signal from the ground. The pulse method was used in testing the automatic ripple fuzes since, when falling vertically, the radiation lobes are essentially horizontal and hence in a poor position for self-operation of the fuze as it approaches the ground. It could safely be assumed that a fuze which could be pulsed in a base-down position would also detonate automatically as it approached its target. If the unit did not cease oscillating, or did not detonate prematurely aloft (detectable by craning the neck to note the presence of a puff of smoke directly overhead), and if it was otherwise operable, it was pulsed several yards above the ground. The landing position was duly noted, and the crew went on to the next round. As the firing load increased, the crews (by

this time greatly expanded over the "whoever he could find to help" stage) were able to keep aloft three active shells, or five inert shells used when testing mechanical quality of tubes alone.

Actual recovery of the units then followed. Posthole diggers were used throughout the program, since no more efficient method of recovery was devised. The truckload of shells (later it became truckloads) went back to Silver Spring for postmorteming. This process sometimes lasted until the early morning hours, but at 7 A.M. the following morning, the next truck convoy started off to the field for another day's testing.

Test results were telephoned from the field to the Laboratory, where they were awaited with intense interest. The conversation was in code (the results were transmitted as a dollars-and-cents market quotation) because of the obvious requirements of security. The technical team had to know the test results at once in order to find out if their guesses had proved correct, or if further changes were necessary to make the fuze or its components function as expected.

Postmortem examinations cannot be overemphasized as a powerful technique to aid in controlling the quality of components and the assembled fuze itself. Every new component, or component from a new manufacturer, was tested and proved in, and quality control standards established and maintained by successive tests of statistically significant samples. Each new variation in fuze design was field-tested as soon as possible, because the pilot lines or regular production lines continued to roll along on best bets already at hand. Thus the VT-fuze for shells became a useful battle device by a long process of trial and error, with the empirical approach far outweighing the theoretical one.

To a great many of the men, the earlier days of the Section T program are indissolubly associated with the exacting drudgery of the test-field and recovery firing. To them, this was "Section T." They will tell of Curly, the caretaker's Chesapeake retriever at Stump Neck. Like any well-trained bird dog, Curly would go wild with anticipation of game to be fetched when the 57-mm. went off—such a big gun would surely bring down big birds! He would race madly out and, as soon as a shell landed, hurry over to the spot and dig away until the next round was fired.

The chances of being hit on the field were very small, but shelters

were nevertheless quite necessary. It was later remarked that the very first shell fired for recovery at Vienna gave very clear indications of what was to come — it landed in a briar patch! Crew members sometimes became careless until a shell landed too close, but the word was that only a direct hit counted. Shells usually dropped in the test area, but a sudden change of wind might carry a shell far afield. Several close calls were occasioned by such erratic flights — the closest less than one yard! One shell intended for the recovery field landed in the front yard of a farmhouse more than a mile away. This extreme example of bad flight was caused by a small piece of lucite, breaking off from the shell nose. Sometimes shells would be floaters — they tumbled, took an unusually long time getting down, and landed in unpredictable spots. One fuze unit detached itself from the shell somewhere along its trajectory, landed just fifty yards from the gun that fired it, and went off on proximity to the ground. The main powder magazine received a direct hit — the shell came through the roof, but dropped harmlessly in the aisle between two high stacks of ammunition.

Perhaps the best hit story concerns the case of the lost shell. The landing spot of a shell was located and marked by observations from three posts. One afternoon after firing was ended, a shell had not been recovered although it had been observed to land in the target area. Sheer persistence revealed that two shells had landed in exactly the same spot — digging deeper finally uncovered the missing shell.

As the fuze moved into mass production, the practical functioning of the units was tested by requiring that a sample of each manufactured lot should be given a standard "down-river" test at Dahlgren. This was the basis of Navy acceptance of units from the different fuze assembly companies, and consisted of firing over water so that as the shell reached the end of its trajectory, the fuze was triggered (rippled) by proximity to the water surface.

A great deal of credit must be given to the Erwood Company of Chicago, Illinois, for the assistance they gave to the Section T technical staff during the early development of the fuze. This small company was not hampered by the routine procedures necessary in a large industrial organization, nor by the academic pace from which administration in a purely research organization sometimes suffers. The Erwood group was characterized by flexibility, a co-operative attitude, quick action, and

its members have been called quite aptly "The Commandos of the VT-Fuze Project."

This company was brought into the program in the fall of 1941 for the purpose of doing odd shop jobs, especially in the fabrication and assembly of special fuze components. Since 1942, all of its time has been spent on Section T projects, with often a hundred individual jobs in process for various parts of the program. Co-operation with Section T began under conditions which keynoted the whole association of the two groups. It is reported that on a hot fall afternoon in Washington, John Erwood came in to talk with Tuve about prospects for an NDRC contract. In peacetime, the Erwood Company manufactured sound equipment and record changers. Tuve was very anxious to establish contacts with groups with industrial experience, since successful laboratory fuze units had been developed, and a pilot line was needed, as well as outside shop facilities for models. In characteristic fashion, Tuve finished the discussion by telling his visitor, "Now, of course, I wouldn't want you to jump into this thing and commit your company for several years without taking some time to think this over — go out on the lawn and think it over for fifteen minutes." Erwood thought it over, and then started right in to assemble amplifiers, using company funds (somewhat to the discomfiture of the stockholders and the bank) until contract funds came through regular channels.

To get containers for a rush job on amplifiers to Washington by the night plane (orders telephoned to Erwood in the afternoon were usually delivered at the Washington Airport early the next morning, picked up and taken to the test field, and postmortemed that evening), they canvassed neighborhood stores for all the salt and pepper cans that could be found. When fans were needed for another special job, Chicago secondhand stores were scoured for old vacuum cleaners. In hundreds of ways, the Erwood group proved masters of improvisation, and thought of red tape as something to tie around Christmas packages. In one day alone, their pilot line changed models three times.

Machine tools were a very critical item throughout the program, and the need for speed was so urgent that the Erwood company found it necessary to purchase whatever secondhand equipment was available — some of their lathes dated back to 1907. The staff of their improvised shop typified the spirit prevalent throughout the country, especially dur-

ing the early stages of the national emergency. The men often made their own dies and tools, thus saving time at critical stages. The wartime emergency operators of lathes and machines in the shop represented a veritable cross section of peacetime occupations. Among them were a contractor for Venetian blinds, a bartender, a window washer, a shoemaker, a food-products salesman, a "grease monkey," a surplus-products broker, a baker, a filling-station attendant, a farmer, a warehouse man, a laundry-truck driver, a barker in a carnival, a Canadian policeman, a trucking contractor, a purchasing agent for a fur company, a counterman in a restaurant, a professional wrestler, and an advertising copy writer. The staff was built around a man whose only machine-shop experience had been in running a model airplane shop — it was evident that the Erwoods had to grow their own experts.

Tuve demanded action and the Erwood Company rarely failed to meet his demands. As the Section T program expanded, the Erwoods added space and personnel in order to continue to handle the needs expressed by the technical men in the Washington group. And when the "Section T" Applied Physics Laboratory went under a special research contract with the Navy, the Erwood Company continued to go right along with them under the same type of contract.

CHAPTER XIII

THE APPLIED PHYSICS LABORATORY OF THE JOHNS HOPKINS UNIVERSITY

A METROPOLITAN traffic jam cannot be broken up merely by removing cars having out-of-state license plates. A more fundamental solution is to build highways with a minimum of intersections, or to use a series of stop lights controlled from a traffic center. In the same sense, the technical jam which had been building up in Section T even before the Navy gave the "full speed ahead" signal on mass production of radio proximity fuzes could not be eliminated by the transfer of work on bomb and rocket fuzes to the Bureau of Standards.

In January 1942, after the first test of factory-made fuzes giving 50 per cent operability at Dahlgren, it was evident that modest expansion of staff and facilities at the Carnegie Laboratory would not be adequate to meet the anticipated needs of the fuze program. The Navy recognized the difficulty of introducing a new and complicated development device into large-quantity production for Service use, in the very short time allowed by war urgency.

Some new organizational setup was needed to provide a mechanism for handling this diverse technical traffic—a "red-ball" highway leading from the smallest room of a research laboratory to World War II battle fronts, crossing the wide plains of testing and quality control, and the rugged mountains of procurement and mass production. The NDRC, established for research and development work only, was not designed to handle industrial production problems on a vast and interlocking scale.

Upon Tolman's recommendation, and after considerable study by National Defense Research Committee officials and by Bush, it was decided to transfer Section T and its activities directly to the Office of Scientific Research and Development. This took place in March 1942. It was also necessary to find another contractor to carry the responsibility for and provide the mechanism for the expansion of Section T

activities. The ultimate successful use of the VT shell fuze in the war can be ascribed without question to finding such a contractor — the Johns Hopkins University, which established an effective administrative mechanism for seeing the job through.

The Johns Hopkins University had been desirous of making additional and significant contributions to the war effort. This opportunity came when Bush suggested to Isaiah Bowman, President of the University, that establishing and administering a central technical laboratory for Section T would fill an urgent need and constitute a very direct contribution to the war program. This type of activity would be quite a departure from the University's normal pattern, since it would involve direct participation, at the Navy's request, in industrial-production problems, and the technical guidance of activities carried on at other universities under OSRD contracts. Bowman considered the matter favorably, and the Johns Hopkins University accepted an OSRD contract. Military security regulations here posed a queer problem. The over-all radio proximity fuze program was classified "Secret," while certain of its aspects were "Confidential." The members of the Board of Trustees of the Johns Hopkins University were not, therefore, told specifically why they were being asked to approve advancing University funds to establish a laboratory "off campus." This problem was met by informing a very few Trustees, after the required clearance formalities, why such a step was necessary, and by delegating the University's contract authority to one Trustee — D. Luke Hopkins. In addition to being Vice-President of the Board of Trustees, Hopkins was designated "Authorized Representative" of the Johns Hopkins University for the Applied Physics Laboratory.

The OSRD contract, dated March 10, 1942, stated that the contractor "shall, with the utmost dispatch and in accordance with the instructions issued by the scientific officer, undertake the necessary preparatory work for and equip, staff, operate and maintain a laboratory or laboratories, together with office, model shop, testing field facilities for studies in experimental investigations in . . . ordnance devices . . . construct, test and modify such devices, . . . procure the development, construction and testing of such devices . . . by contracts with others, as directed by the scientific officer and cooperate . . . with other agencies concerned." Coupled with this broad directive was the utmost possible

fiscal freedom, it being tacitly recognized by all contracting parties that "war is directed waste" and that time, not money, was of paramount importance.

The Applied Physics Laboratory started its existence in a used-car garage. The sign "Used Cars" was kept intact in the interests of security, and many a seeker after cars or repairs was turned away by the armed guards patrolling the entrances of a building surrounded by alarm fencing. The laboratory continued to expand. The demand for more space was incessant. Additional buildings were constructed and the old ones revamped; in 1944 the laboratory appeared as shown in the lower picture facing this page.

In mode of operation, the Applied Physics Laboratory fulfilled all of Tuve's desires. The greatest possible support was given to individual initiative, consistent with common sense (and even this was sometimes tempered to meet the war situation). Direction of the technical activities of the Applied Physics Laboratory and of the many associated contractors was vested in Tuve as Representative of the Contracting Officer of OSRD and responsibility for the Government also was charged to Tuve as Chairman of Section T. Administrative matters for JHU as contractor were handled by Hopkins. Rear Admiral (then Commander) W. S. Parsons was Special Assistant to the Director of OSRD in administering the Navy funds allocated to OSRD for Section T and in expediting the fuze program.

Funds in limited sums were made available immediately to responsible supervisors in the laboratory. Larger sums required special approval which, however, could be, and was on occasion, obtained in a matter of hours.

Nothing can illustrate the spirit with which Tuve infused his workers better than the following representative set of "Section T Verbal Rules," or unwritten informal "operating rules." The elastic organization of the Applied Physics Laboratory allowed full scope for the operation of these rules. Under this code of operation it was surprising to note to what degree the initiative and personal responsibility of staff members expanded; the men were made to feel a very real, personal responsibility in the war and in the project which led, in some cases, to an almost unbelievable personal loyalty to Tuve and to the project—the two were indistinguishable to most of the workers.

Operating Rules

1) "I don't want any d—n fool in this laboratory to save money, I only want him to save *time*."

2) "We don't want the best unit, we want the *first* one."

3) "There are no private wires from God Almighty in the lab that I know about,—certainly none in my office."

4) "The primary duties of any supervisor are initiative and forethought, he is supposed to make his team do the work."

5) "Any function or area of a total job which can be described and manned should be *assigned*. Articulate your work."

6) "The trouble is always at the top."

7) "The Navy says so! Who is the Navy? That was only the opinion of the man you were talking to."

8) "A good short paper in your hand at the right instant and place is a marvelous hatchet for getting action. 'Red tape' is a tool; use it, but use discrimination in your paper work."

9) "Responsibility and authority always have the same boundaries; this is axiomatic."

10) "Our moral responsibility goes all the way to the final battle use of this unit; its failure there is our failure regardless of who is technically responsible for the cause of failure. It is our job to achieve the end result."

11) "Run your bets in parallel, not in series. This is a *war* program, not a scientific program."

12) "The *final* result is the only thing that counts, and the only criterion is, does it work *then*."

13) "Shoot at an 80% job, just a passing grade, we can't afford perfection. Don't try for an A, in a war a D is necessary and enough but an F is fatal. Don't forget that the best job in the world is a *total failure* if it is too late."

It is obvious that only an enlightened organization which held broad policies with respect to formulating problems and attacking these with "self-generated initiative"—and which allowed technical men possessed of vision and a trenchant grasp of the over-all problem a free

rein in such an attack — could sponsor such "operating rules." Under such operating conditions, men could and did feel a personal responsibility in the war — it was very much their war, and they could work with the feeling that what they did or did not do could measurably affect the outcome of the war.

CHAPTER XIV

THE VT-FUZE — THEORY, EVOLUTION, AND EVALUATION

THE RADIO proximity fuze as used against the German V-1 robot bombs in England, in which campaign the fuze played a major role after it had already proved its effectiveness in Pacific naval actions, can be taken as typical and its operation described in some detail.

Let us first consider how a proximity fuze must function in order to be effective. The signal by which a target makes itself known to a VT-fuzed projectile is generated continuously by an oscillator in the nose. Detection of the reflected wave from the target sets up a beat frequency in the plate circuit of the oscillator which, when fed to the grid of a thyratron after being amplified, triggers the firing circuit and initiates the explosive chain. With VT-fuzed ammunition, the chief requirement as far as gun crews are concerned is that the trajectory of the projectile must pass within a certain radius of the target.

If the projectile misses the target by more than the radius of action of the VT-fuze, it must destroy itself so that the shell will not trigger on approach to the ground, spraying lethal fragments over friendly territory. The fuze must also be safe to handle, and must not arm until it is a safe distance from the gun muzzle. Considerable time and effort were devoted to the problems of safety and self-destruction, not only in developing special devices for these purposes, but in supervising their manufacture in large quantity and controlling quality within close tolerances.

In a typical VT shell fuze, the radio station is encased in a tough plastic nose cap which insulates it electrically from the rest of the shell. The oscillator, coupled to a small antenna cap embedded in the plastic, which acts as the exciting element, transforms the entire shell into a radiating system.

The phenomenon of interaction of the radiated and target-reflected signal can be regarded as a standing wave pattern cut off by the pro-

jectile, thus producing the ripple signal; it can be regarded as a Doppler phenomenon, the shell "hearing" slightly higher frequency than it emits as a result of the relative velocity of shell and target-reflector; or, as a variation in the radiation resistance of the antenna as it moves relative to a reflecting object.

Viewed as a Doppler effect, the frequency of the reflected wave V_1, in terms of V_0, the transmitted frequency is

$$V_1 = \left(1 + 2\frac{V_g}{c}\right)V_o$$

where V_g is the velocity of the shell relative to the target and c is the velocity of sound. Thus

$$\Delta V = V_1 - V_o = \left(\frac{2V_g}{c}\right)V_o$$

From another standpoint, when the antenna of a radio transmitter is in the neighborhood of a reflector, the reflected wave train induces electromagnetic frequencies in the antenna which may either aid or oppose the flow of current supplied by the oscillator, depending upon their phase difference. The radiation resistance, R, becomes thus a function of the distance, x, between target and shell, and is represented approximately by

$$R = R_o\left[1 - \frac{1}{x}\sin\left(\frac{4\pi x}{\lambda}\right)\right]$$

where R_o is the free space radiation resistance and λ is the wave length of the radiated signal. Thus radiation resistance completes one cycle of variation each time the distance changes by $\frac{\lambda}{2}$, the amplitude of the variation increasing hyperbolically as x decreases. The variations in oscillator plate current induced by the variable resistance produce an oscillating voltage drop across a high resistance in the plate circuit which, when amplified by the two-stage pentode amplifier of the VT-fuze, is fed to the thyratron grid. When the amplitude of the voltage variations becomes large enough, the thyratron triggers and discharges the firing condenser through the squib, or cannon primer. Circuit details are still highly classified; many interesting and significant variations of the original design have been made, particularly

to improve the sensitivity of the fuze to signals characteristic of probable targets.

Physical scientists are accustomed by training to isolate variables in performing experiments. They attempt to control all the variables except one, thus permitting the effect of altering any specific variable to be unequivocally determined.

The development of the VT-fuze represented a totally different picture. There was no time to be "scientific," and the fuze design evolved from a sea of unknown variables. No one could hazard a guess as to how many variables would be involved, or what they actually were. In one fuze model, for example, a new method of waterproofing, by covering the nose with wax, was adopted. It was impossible with the urgent time scale facing Section T to fire a large number of fuzes waterproofed in this way, and an equal number not waterproofed, holding everything else constant, in order to determine what the effects would be. It was not even known whether a wax covering would be a variable affecting the functioning of the fuze. As a matter of fact, in the waterproofing experience, statistical separation of variables during many tests actually indicated that, with other factors held constant, waterproofed fuze units gave better scores — i.e., there were more properly functioning units. This was sufficient reason to continue coating the noses with wax even after another type of waterproofing material was developed. With the program in an emergency status, a particular brand of chewing gum bought in a particular store by a redheaded messenger boy on alternate Tuesdays would have been slapped on the projectile noses if there were even slight indications that this would have improved test scores!

Physical effects have physical causes. The function of the waterproofing wax was simply this: in tests with certain guns where the projectile velocity was especially high (as when the initial temperature of the propellant was higher than average), the solder connection to the antenna nose cap became sufficiently softened to allow the cap to vibrate or shift during flight — and the result, a premature action of the fuze!

Propellant charges varied from time to time; gun wear was progressive; variations in tolerances of the many fuze components certainly contributed to the total of variables; gun temperatures changed

from time to time; the amount of humidity in the air varied from test to test (perhaps this affected the leaking off of space charges — were there space charges? could they cause trouble?). Besides, it was necessary to test this new batch of tubes today, and a neoprene gadget instead of rubber, and a .05 condenser had been put in to replace a .005 condenser to get a different circuit response. There had been a mistake and a molded-in antenna cap was being used instead of the one specified! Today's batch of batteries was from a southern plant and they had been having troubles with humidity. Also the "K" ampules were in this same batch — they were new and still being tested for better side drops. The mercury switches were timed a little differently; a new filter was in; the off-center measures on these test projectiles were a little worse; the auxiliary detonator might also give trouble. The thyratrons were from "week X" production and were probably better "strikers" — but what about assembly pressures at the plant? And then the amplifier band pass was slightly different!

What, test only one variable at a time! Wax waterproofing instead of a luting compound? If the score was different, it might be due to gun effect, rain effect, the *mu* of the pentodes, a bad day on the assembly line, or any one of a hundred different little or big difficulties. Testing for one variable was hopeless in the drive to get a working fuze to the Fleet in the shortest possible time.

These were the growing pains of the VT-fuze. After the basic design had become available in all essentials, the mechanical details were achieved only after seemingly endless experiments, tests, and modifications. From the original Mark 32 Navy fuze, the design moved logically to smaller size, more complex circuits, and greater mechanical stability. In the pilot-line model of the Mark 32, the nose was of transparent lucite and a trimmer condenser was incorporated, which could be adjusted by a set screw through the lucite block. The trimmer was originally considered essential — who ever heard of oscillators that did not have to be tuned before final use? However, the first production model of the Mark 32 omitted all adjustable features such as the trimmer, and the transparent nose was replaced by a black lucite one for security purposes.

This early Mark 32 contained an oscillator, separable as a unit from the rest of the fuze, which was connected to the amplifier. The ampli-

fier plugged into a dry battery, which in turn was connected to the rear fitting, a metal housing containing the safety-delay clock mechanism and the firing circuit. The latter consisted of a comparatively high-capacity firing condenser which discharged through the thyratron-squib combination. The entire fuze, exclusive of the auxiliary detonator, was 10 inches long and 2½ inches in diameter. The oscillator, amplifier, battery, and rear fitting were encased in a steel housing or "can" which was threaded to screw into the projectile.

Smaller-diameter fuzes (the Marks 33 and 41) had to be developed for British and U.S. Army projectiles. The Mark 33 was essentially a smaller version of the Mark 32, except that the reserve type of battery replaced the dry battery. The Mark 41 was a shorter version of the Mark 33 as a result of eliminating the mechanical safety clockwork and inserting an annular firing condenser which circumscribed the amplifier. The necessity for using reserve batteries in the British fuzes (due to inability to design a small-enough dry battery which would operate reliably) led to supplying the British Navy with units having a longer shelf-life than those used by the American Navy. The U.S. Fleet used VT-fuzes with dry batteries until nearly a year later, when the introduction of the reserve battery and certain other modifications resulted in the model known as the Mark 40. This unit was the same size as the Mark 32, but was an improvement in general performance and storage life.

A significant change in design occurred in 1943 when the oscillator and amplifier were combined into a compact unit. All component parts including four or five miniature radio tubes, were compressed into the volume of an ordinary ice-cream cone. This step was made possible by the development of smaller components and was the result of increased manufacturing experience as well as laboratory experimentation.

The reduction in size thus made possible additional space for high explosive in the shell itself. The length of this Mark 45 fuze is 8 inches, with a diameter of 1¾ inches.

The wide application of VT-fuzes to a variety of howitzer projectiles was made possible by the development of the more compact Mark 45, in which the oscillator, amplifier, and firing condenser were incorporated into the space previously occupied by the oscillator alone. With the miniature safety devices, the Mark 45 type of fuze became no

longer than a half-pint milk bottle and somewhat narrower. More than 10,000,000 Mark 45 units for howitzer use were manufactured. Howitzers do not have the high acceleration of the antiaircraft guns, nor need they be as sensitive. Further, since a howitzer fuze operates only near the end of its trajectory, a longer arming period can be used, this cutting down materially on early premature actions. A triode rather than a pentode amplifier can be used, and the lower acceleration does not place as severe stress on fuze parts. These factors combine to make howitzer fuzes the most consistently good operating units ever manufactured.

The development of the Mark 45 was also contingent upon the development of a satisfactory reserve energizer. When the latter became available in the summer of 1943, the signal was given for full-scale manufacture. The Army requested high production rates to insure large backlogs to be strategically located in the ETO for immediate use when the Combined Chiefs gave the word.

A combination fuze, known as the Mark 53, eventually became the basic design for VT-fuzes used until the end of the war. The use of ethyl-cellulose plastic for the nose was a significant contribution to the mechanical success of the fuze, since this material combined toughness and desirable dielectric properties. This basic type of fuze proved very effective against Japanese kamikaze attacks, and in Army howitzers, for which the greatest number of VT-fuzes were manufactured. Fuzes were designed for a total of some twenty types of guns, ranging from the 3-inch to the U.S. Army 240-mm. howitzer, and designs for a number of additional gun projectiles were in process at the war's close.

If a radio proximity-fuzed shell does not burst in a lethal position with respect to a target, it is obvious that its effectiveness is nil, or even negative. The latter effect can occur, for example, if all the bursts are late, causing shell fragments to spray out behind the plane. In this case a pilot could fly through a major barrage with safety, whereas with time fuzes there would be at least some chance of hitting the plane. It is thus obvious that a knowledge of the fuze sensitivity pattern (the locus of points about a target at which VT bursts occur) is necessary for an evaluation of its effectiveness in action. Additional information is also needed for a proper evaluation of VT-fuze operation against aircraft — the accuracy of fire-control directors used with VT-fuzed shells; the

fragmentation pattern of the shell itself; and the vulnerability of types of aircraft to shell fragments. Fire control is of especial importance since, unlike time-fuzed fire, the VT operation is a function of trajectory-to-target distance. The fuze sensitivity pattern must match the fragmentation pattern, while vulnerability to shell fragments determines the probable number of bursts on target necessary to inflict crash damage.

The fuze sensitivity pattern is of great importance. The tests at Parris Island provided the first data along these lines. However, the method of testing against drones was prohibitive in time and effort to secure the routine detailed studies of fuze sensitivity patterns. Each fuze model had to be tested against several types of targets and in various aspects to these targets. Repeated tests on each model were necessary to check fuze sensitivity changes which would not be revealed by the routine acceptance firing tests. Operation of these units over water gave little indication of their functioning against a full-scale target.

Though it was realized early in the work that full-scale pattern testing was the real answer, a contract was made with the University of Michigan for the experimental determination of fuze sensitivity patterns using scaled-down models, and for other analytical work. This contract was continued throughout the war, and the University of Michigan analysis group under D. M. Dennison exerted a highly valuable influence in guiding fuze developments, all the more helpful because it came from a distant campus removed from the confusions of the Washington scene.

Determination of the sensitivity pattern was accomplished by the use of a model airplane (1/8 scale) covered with copper, which ran on a rope between two sets of tall poles. A stationary oscillator representing the VT projectile was mounted on a light bamboo pole, and as the model traveled along the rope, the responses of this oscillator were recorded by galvanometer readings in an observation shack some distance away. This time-saving method permitted the determination of numerous VT sensitivity patterns by September 1942, and was used by Crane — again back at Ann Arbor — until V-J Day.

As the fuzes went into large-quantity production late in 1942, it became advisable to use full-scale pattern testing. Ideally, this called for a

target suspended in free space. A close approximation to this was achieved by suspending targets between two wooden towers, 250 feet high and placed 400 feet apart at the New Mexico Experimental Range. This large range was established in the spring of 1943, under the direction of E. J. Workman, former Chairman of the Physics Department, University of New Mexico. Workman had been associated with the Section T program almost from the beginning, and his group had developed several types of safety devices for the fuze. Familiarity of this staff with the VT-fuze, and access to sufficient land for establishing a large test field, made it appropriate to have this work carried on by the University of New Mexico under an OSRD contract. Data secured at the New Mexico Experimental Range proved invaluable throughout the VT program.

Targets of all types were suspended from the towers and fired upon with VT-fuzed projectiles, including Japanese Bakas, German V-1 buzz-bombs, and even the transpacific balloon. With the exception of the latter, which was furnished by the Japanese, all models were constructed in the New Mexico shops. Smoke powder, substituted for high explosive, permitted the bursts to be photographed without injury to target or towers.

All sensitivity pattern tests were carefully recorded from several observation posts, thus permitting the construction of a three-dimensional pattern. The Range itself is located between the Sandia and Mongano Mountains, whose foothills form an excellent backstop for projectiles. The staff at the range, numbering over 100, included bona fide cowboys who maintained security by patrolling the otherwise completely open territory.

A series of airplane vulnerability tests under the direction of D. M. Dennison was carried out on this Range, in which the University of Michigan and New Mexico groups co-operated. Numerous airplanes of different military types, mounted in various positions a few feet off the ground, were the targets of fragments from projectiles detonated at selected points about the target. This was accomplished by placing a suspended board at the selected distance and aspect to the plane.

The damage thus inflicted was carefully assessed and formed the basis of a comprehensive study of the vulnerability of different types of aircraft and their crews to fragments from VT-fuzed shells.

The third type of data needed for VT-fuze evaluation — the performance of fire-control directors — was obtained through the studies of Dennison's analysis group and the large group at the Applied Physics Laboratory in Silver Spring which was engaged in the development of the Mark 57 director system.

The combination of these data on sensitivity patterns, vulnerability, and fire control gave the technical men an accurate estimate of the expected performance of the fuze many months before reports from Naval actions began to accumulate in sufficient quantity to corroborate their predictions. The predicted effectiveness was in accord with facts, as later detailed mathematical analysis of thousands of action reports clearly showed. It was a source of amusement as well as irritation to Section T personnel to have the predictions of fuze performance based on analysis received with skepticism by those unfamiliar with analytical methods. The long introductory period of the fuze into the Fleet can be ascribed in some measure to the reluctance of Navy personnel to accept the statements contained in such pre-battle evaluations. In all fairness, however, it must be indicated that previous experiences accompanying the introduction of new weapons, with the bugs usually associated with early models of such devices, probably account for this long indoctrination period, and the reluctance to place too much confidence in a device untried by action.

CHAPTER XV

ADAPTING THE FUZE TO BRITISH AND U.S. ARMY GUNS

VERY EARLY in the program, VT-fuzes for the protection of the British Navy were placed second in priority only to the U.S. Navy, and ahead of the needs of the U.S. Army. Lend-Lease supplies of this material flowed to England from the summer of 1943 to the end of the war. The cause of the two navies was a common one, and once the Mark 32 for the U.S. Navy was successfully in production, the laboratory concentrated on the Mark 33, and later the Mark 41, for His Majesty's ships.

British research and development work on the VT-fuze never expanded into British production, owing largely to the physical proximity of the war and the lack of adequate, protected manufacturing facilities. In recognition of this and the common cause of the two nations, the U.S. Navy did not hesitate to let full-scale production contracts just as soon as these special fuzes looked promising to the Section T engineering staff.

It was essentially a separate problem. The standard 2-inch-diameter U.S. Navy fuzes could not be used in British Navy 4.5-inch and 4-inch projectiles, which required a 1½-inch-diameter fuze. This extra half-inch was, however, equivalent to another order of magnitude. An entirely new safety clock had to be developed to parallel the 2-inch clockwork of the Mark 32. The radio oscillator and amplifier had to be redesigned. But rising above these difficulties was the problem of the power supply, or battery, a difficulty which reached emergency proportions in the spring of 1943, threatening to stop the entire small-fuze program and which thus deserves to be mentioned in some detail.

The 2-inch dry battery, already a gem of compactness, could not be further compressed satisfactorily, and though a 1½-inch battery was manufactured in some quantities, it never became a standard unit. This forced the engineering plans of Section T; for some time it had been agreed that sooner or later the dry battery would have to be replaced

with a small, wet battery which, however, would remain inoperative until a fraction of a second after gunfire. Such a unit was designed by the engineers of the National Carbon Company, and the Applied Physics Laboratory. A stack of annular plates provides the principal structure; in the hole of this cylindrical doughnut rests a glass vial in which the electrolyte is held captive until the glass shatters under the impact of gunfire. The electrolyte is centrifugally distributed by the spin of the shell, and in a tenth of a second, in some cases, enough voltage is developed to activate the radio transmitter.

The reserve battery, as this unit is called, is in itself a technical problem comparable to that of the radio fuze. To insure proper electrolyte distribution, manufacturing tolerances must be exceedingly small; leakage produces short circuits within the battery, and any fluctuations in current flow to the radio parts of the fuze can upset the electronic balance, thus triggering the circuit, causing the VT-fuze to "premature."

The battery must be operable over a wide range of temperatures and the vial, or ampule, must be rugged enough to withstand rough handling, but sufficiently delicate to shatter instantly upon gunfire, or setback. At one stage of the program a paradox developed — a particular type of ampule failed to shatter at setback when the forces were axial, yet broke when dropped sideways only a few inches onto a steel anvil!

A memorable emergency occurred in Section T history in the spring of 1943. Tuve had not waited for an acceptable reserve battery to signal full-scale production of the 1½-inch British fuzes. (Nor, for that matter, for acceptable smaller safety clocks for these units.) Fuzes piled up on the shelves at the RCA assembly plant at Bloomington, Indiana, but the production line was pushed on despite the specter that all the fuzes might have to be destroyed (by crushing in huge presses, the means demanded by the secret classification of the project for doing away with unwanted, but secret, parts); the rear-fitting production line kept rolling, and so did the reserve-battery plant the National Carbon Company had built especially for this purpose at Bennington, Vermont. The fuze for the British represented one of the largest, if not the largest, American manufacturing gambles on record. "War is directed waste," Tuve had frequently said. The reserve-battery program, involving at times the deliberate scrapping of production-line output day after day until

"we learned how to make batteries by making them," was a trenchant example of this.

The gamble proved successful. The British Navy received its first VT-fuzes (Mark 33) in the summer of 1943, just over a half year after the U.S. Navy had begun using the Mark 32. The hurried development of the reserve battery for the British small fuzes opened the way for an entire new family of small fuzes, the Mark 45 type, which combined the oscillator and amplifier into one unit in the projectile nose tip, and substituted the small mercury switch safety device for the cumbersome clockwork.

It was these latter fuzes which, in the fall of 1943, went into full-scale production for U.S. Army use, and which comprised by far the majority of VT-fuzes manufactured during the war. The demonstration of a successful VT-fuze for Army howitzer and AA shells precipitated demands for quantities which belied any understanding of the problems of production. Even when reduced to realistic terms, the program called for drastic expansion of production facilities, particularly for reserve batteries. Construction of new factories from scratch at that stage of the war involved a careful evaluation of the labor market, as large sections of the country were already saturated. Winston-Salem, North Carolina, was chosen as a likely spot. Many times larger than the Bennington, Vermont, plant, it had an army of workers, as intensely loyal a group as found anywhere in the VT program, though drawn from all levels of this North Carolina community: groups of Negroes who sang in unison to the rhythm of the punch presses, girls who frequently insisted on going barefooted, and women of society who had never touched machinery before but who came to regard working on reserve batteries (though innocent of the term or the function) as their worthiest contribution to the war effort. By January of 1944, this plant was producing 430,000 units per month, doubling this number a year later.

On V-J Day, the various plants of the National Carbon Company, of which the Winston-Salem plant was the major contribution, were producing at a daily rate of 75,000 units, the Eastman Kodak Company, 23,000, and the Hoover Company, 5000.

The substitution of the spin switch for the clockwork type of rear fitting was also first initiated in a fuze manufactured solely for the

British Navy, this time for the 4-inch gun. For the first time in this model (Mark 41) the firing condenser was brought from the rear fitting and, in annular form, wrapped around the amplifier, the clockwork abandoned and replaced by two mercury switches and one spin switch, all encompassed, with the squib, in a unit ¾-inch thick.

The first overseas shipment of this fuze was made late in the fall of 1943. Thus the production contracts for the British fuzes, placed with RCA and with the Eastman Kodak Company late in the fall of 1942, bore fruit, but only after a long touch-and-go battle on the fuze, the battery, and the rear-fitting production lines.

A grave difficulty with British fuzes which sprang up to plague Section T and its contractors was the "worn gun" effect. Unlike American ammunition, British ammunition is fixed, i.e., the propellant has always the same position in the gun. As the gun rifling wears, the projectile is propelled through the smoothly worn part of the gun and encounters the rifling farther up along the barrel with considerable force. The "negative setback" and the "side-slap" experienced when the projectile first strikes the rifling subject the fuze to forces not on the program. Much additional engineering work, involving much repeated testing, was necessary before the British fuzes were mechanically reinforced to cope with the strain from an unexpected direction.

The success of the British fuze program is due to several people — to Section T leaders, who frequently anticipated British needs before formal request was received through channels; to E. O. Salant, who headed Section T British liaison; to E. D. McAlister, who directed the Section T group engaged in the work on British VT-fuzes; and to Neil Dilley, who headed the special project on the clockless, shorter Mark 41. The fine work of the RCA and Eastman Kodak Companies made the actual production possible.

VT-fuzes for the British Army howitzers, and for their 3.7-inch guns which were so effective against the V-1 robot bombs, are dealt with in another section. As with the American Army, these exceeded in quantity the Navy fuzes.

CHAPTER XVI

THE VT-FUZE IN ACTION

THE VT-FUZE found wide application on land, on sea, and in the air. It was first used in the Fleet against aircraft, long before security considerations allowed its use on land.

It is far more difficult to give a graphic account of the over-all use and effectiveness of the VT-fuze in the Fleet than it is for its use on land. Each naval engagement is a separate operation and one must patch together comments from a large number of individual action reports to get a complete picture. In the buzz-bomb campaign, for instance, the action was far more intense and more localized. The effect of VT-fuze action on land was graphically expressed by records of buzz-bombs destroyed or battalions wiped out, which is much more impressive than a report of a torpedo bomber being prevented from accomplishing its mission. In the latter case, one cannot say that a torpedo bomber destroyed or driven from its intended course affected the winning of a battle or saved a certain number of lives.

When, however, one scans numerous individual naval action reports, the over-all picture of the VT-fuze in action is impressive. There is great similarity in such action reports. Quotations from a few of them will suffice to illustrate. For instance, in relation to action off Makin Island, the Commander of the Seventh Fleet reported: "Four shots were fired, using the Mk. 32 on the last two. The third shot was observed to burst immediately under the tail of the enemy plane which immediately nosed over and crashed into the sea." In the same report describing action off Tulagi, it is stated, "As he passed astern the third burst of a 5″/38 AA (VT-fuzed) appeared to have completely surrounded him, for as he emerged from this burst he was smoking and crashed." A longer and more graphic report describes the action of the U.S.S. *Daly* (*DD-519*) in reporting the assault and occupation of Okinawa in April 1945: "Some of the planes broke away and commenced long glides, unmistakably of the suicide brand, towards this ship and were taken under fire by all weapons. One, caught in a with-

ering stream of 40 and 20mm fire, burst into flame and crashed 100 yds. from the ship. The second was hit squarely by a 5"/38 projectile and completely disintegrated. This was the most striking example of the effectiveness of this weapon ever witnessed by this command. The remainder of the planes were still moving off and around the stern. The rudder was thrown hard right to take these planes under fire. The third plane commenced a long shallow glide, and, his wing shot off, he 'splashed' about 200 yds. on the starboard. . . . The fourth plane started from a greater altitude and headed straight for the bridge in a steep dive as the third plane was about half way down. This plane was hit repeatedly but never wavered a bit. When he was about 1,200 yds. away, the rudder was thrown hard left again and the guns kept blazing until the plane burst into flames just over the #2 torpedo tube, lost his wing and crashed with a large bomb, which exploded about 25 yds. off the port side. Immediately following this, a fifth plane came in low from astern and was 'splashed' at about 700 yds. by our fantail 20mm guns. The damage sustained by this vessel — the Medical Officer and two enlisted men were killed and there were fifteen seriously wounded."

The Japanese themselves paid an implied tribute to the growing strength of Navy antiaircraft attacks. In 1942, there were many individual attacks, Japanese planes approaching very close to our ships. In 1944 and up to the advent of the concerted suicide attacks, Japanese aircraft attacks made many fewer close approaches and resorted in much greater measure to evasive tactics. The launching of suicide attacks again allowed full use of VT-fuzed ammunition. The pre-use analysis and the results of naval actions agreed on the ability of VT-fuzed shells to bring down aircraft at various ranges from 500 yds. to 14,000. The value of the fuze stands out boldly in the light of the final evidence—action reports.

Perhaps the most spectacular over-all use of VT shell fuzes came after their first release for use on land to meet the threat of German "vengeance" weapons.

Like something out of H. G. Wells, a new phase of warfare opened just a week after D-Day — the Germans launched their first robot V-1 bomb, the dreaded buzz-bomb, which had been darkly forecast by the Allied concentrated bombings of launching platforms, reported in

the press. The human element was gone; a machine alone sped through the skies to deliver death and destruction at its mechanically appointed time, and no pilot braved a flak-infested sky to bring it through. At a speed of about 375 to 390 miles an hour, and flying low, the V-1 was a difficult target.

Small wonder that the early brunt of allied defense was assigned to the RAF, which had so valiantly won the first Battle of London, to bear as well as it could. The inadequacy of land antiaircraft was notorious, and a secondary defense role was assigned to it. After a full month of mounting buzz-bomb warfare, the defensive tactics were radically altered and all available 90-mm. and 3.7-inch antiaircraft batteries were moved to the Channel coast. The death-dealing buzz-bombs had met their match and the campaign its eventual doom.

This change in tactics was no accident, nor was it an act of desperation. The story behind it is one of the outstanding scientific — primarily American — romances of the war. The buzz-bomb was defeated by a combination of three OSRD-produced weapons — the VT-fuze, the M-9 director, and the SCR-584 radar. In combination, these three fully met the challenge of the buzz-bomb and robbed Germany of what at first seemed almost certain vengeance — victory. The VT-fuze played one of its most specific as well as spectacular roles during World War II, and its worth as an antiaircraft weapon was most clearly demonstrated.

In late 1943, Intelligence reports brought the first hint of the impending use of the buzz-bomb or V-1 rocket. Sketches from British services were received at Section T which indicated fairly well the construction and nature of the V-1, and within a few days after the receipt of this information, a V-1 mock-up had been constructed in the shops at the New Mexico Experimental Range. This model differed from the actual bomb only in some few details, it was discovered later. Long before any official requests came from the British, Section T had obtained sensitivity patterns of VT-fuzes against the model and had begun the manufacture of special fuzes, based on the New Mexico information, to meet the buzz-bomb when it came. Test results indicated that the VT-fuze would be many more times as effective against the V-1 as time fuzes; actual battle results indicated that this factor was five to one.

It is of importance that Section T took the responsibility of recommending the manufacture of these special VT-fuzes to be used against

the buzz-bombs. This was especially fortunate, since it was discovered later that the reports and recommendations resulting from the Section T trials at New Mexico, though sent to the United Kingdom, did not reach the British Anti-Aircraft Command. Indeed, that Command had not even been informed of Section T's successful development of a fuze for the 3.7-inch shell. Thus, when an urgent British request for 3.7-inch antiaircraft fuzes for use against the flying bomb arrived in 1944, engineering and testing had been completed and full-scale manufacture could begin immediately. It should be pointed out that an informal request for the development of VT-fuzes for British Army antiaircraft guns had been made during the summer of 1943 by Sir Frederick Pile, Commander in Chief of the Air Defense of Great Britain, to E. O. Salant, who was charged with Section T British liaison. This and the active co-operation of British Navy representatives at Section T, A. F. H. Thomson, Captain (then Commander) A. M. Hutchison, R.N., and William McPherson, made it possible for Section T to anticipate the need for, and supply fuzes for British guns, which otherwise would not have been ready in time to play a decisive part in the campaign against the flying bomb.

Anti-buzz-bomb fuzes began to arrive in the United Kingdom in large quantities shortly after the first flying bombs were launched against London in mid-June of 1944. Salant returned to England at the end of July.

His primary mission was to advise in the use of the VT-fuzes against the V-1 targets and to advise the Section T technical team in the United States what was needed to improve the fuze. At the beginning of the flying-bomb campaign, the RAF was the first line of defense, due partly to delays in establishing the coastal gun defenses with all the necessary fire-control equipment and the VT-fuzes, partly to a natural confidence in the air force and partly to a not unnatural hesitancy to base so important a defense upon a new weapon.

Section T, which had ample test evidence of what the VT-fuze could do against airplanes, had recommended from the beginning that antiaircraft batteries equipped with VT-fuzes should constitute the outer defense ring of London.

Soon after Salant reached the United Kingdom he joined with Captain J. P. Teas, Jr., of the Ordnance Department, U.S. Army, formerly

associated with Section T as a National Carbon engineer, in activities to educate the coastal gunners in handling the VT-fuze and in killing certain superstitions about its behavior which were severely limiting its use. Soon the robombs began to fall with gratifying, but never monotonous, regularity. The following table indicates what a difference this made as far as the London civilians were concerned:

For the last four weeks of the now famous 80 days of V-1 attacks, the record of antiaircraft was as follows:

1st Week — 24%	of	all	targets	engaged	were	destroyed.
2nd Week — 46%	"	"	"	"	"	"
3rd Week — 67%	"	"	"	"	"	"
4th Week — 79%	"	"	"	"	"	"

The performance of VT-fuzes in the London buzz-bomb campaign is a matter of record. The dramatic effectiveness of the VT-fuze is amply recorded in reports of the official U.S. Army observer. An excerpt from one report states: "The first employment of VT fuzes against the enemy came with the first launching of the V-1 flying bombs against London on 12 June 1944. The antiaircraft at first played a minor role in the defenses, being allowed to fire only when the weather was such that fighter planes could not operate. . . . By the first of July, it became evident that the defense system could be improved by allowing antiaircraft firing VT fuzes to engage targets without interference from fighter planes. Consequently, during the second week of July all anti-aircraft weapons were moved to the Channel coast where the fields of fire were mainly over ocean areas and where the . . . burst would not be dangerous to civilian personnel. From mid-July until mid-August, when the main V-1 attack ceased, some 500 heavy anti-aircraft guns were free to engage all flying bombs approaching the coast. These weapons included five battalions of 90mm guns operated by U.S. Army personnel. The remainder were British 3.7″ batteries. . . . 90-mm guns began employing VT fuzes from the day they were moved into position. . . . By the first week of August, practically all heavy weapons on the Channel coast were employing VT-fuzes."

In the last stages of the London campaign some 80 per cent of all targets engaged were destroyed. The last day in which a large number of V-1's were launched against England, out of 104 detected by early-

warning radar, only 4 reached London. On a copy of an official report of the HQ AA Command on antiaircraft defenses against the flying bomb, General Sir Frederick Pile, Commander in Chief of the Air Defense of Great Britain, has written, "With my compliments to OSRD, who made the victory possible."

By September 1944 the flying bomb had been decisively and overwhelmingly defeated in the London campaign. The Germans now turned it, however, towards Antwerp and Brussels. The capture by Montgomery of the great port of Antwerp, vital to Allied supply lines, precipitated a hasty retreat by the Germans in that city, leaving, as a result, dock installations almost intact. They pinned their hopes on the buzz-bomb to deny the Allies the use of the port. The first buzz-bomb against Antwerp was launched on October 26, 1944. VT-fuzes which had been especially released for the London campaign were not released for continental use inasmuch as occasional duds might be recovered by the enemy. From late October until mid-December only time fuzes could be used, and because of very well organized defenses were able to bring down as many as 40 per cent of the targets engaged. These antiaircraft teams, however, had learned of the spectacular success of the VT-fuze in England and as one of the officers in charge put it, "literally cried and begged for VT-fuzes." Requests were carried to the highest quarters. The VT-fuze was released for this use in mid-December, the gunners in the Antwerp defenses having been given a week's indoctrination by firing at wire-covered balloons. At this season, bad weather was almost continual and much firing had to be done entirely blind by radar direction. The search radar would pick up a V-1 at about 20 miles, a scarce three or four minutes away. Tracking the invader until it was within firing range, the batteries then opened up with VT-fuzed firing. Often the buzz-bomb was destroyed without ever having been seen by the crew — at other times it was seen briefly as it bore through low scudding clouds. Firing opened at 12,000 yards when the buzz-bomb was almost exactly a minute's firing away. Sixty seconds was all the time available for the VT-fuze to do its job, and the great majority were brought down; especially towards the end of the campaign better than 90 per cent of the targets engaged were destroyed. During the many weeks of the campaign, the crews made sport of their inhuman targets, regarding each bomb as a pitched ball, and each time a battery engaged

its target as a "time at bat." There were fair and foul areas and rules as to which target was a fair one for a given battery to engage. As many as 30 and 40 buzz-bombs were launched against Antwerp each day, usually at daybreak and after sunset.. The havoc which this diabolic weapon could have wrought in Antwerp and London, had it been relatively unchecked as the Germans hoped it might, be by virtue of its great speed and small size, stirs the imagination, but in the matching of wits and weapons, American science, industry, and the military, with the threefold team of radar, gun directors, and the VT-fuze, beat decisively this creation of much-vaunted German scientists.

The third, and by far the widest application of the VT shell fuze, came with its release for general land warfare, in December 1944.

The deadliness of the VT-fuze against ground troops was first demonstrated at Fort Bragg, North Carolina, on the effect field of the Field Artillery Service Board. Wooden planks were placed horizontally or vertically on an open field or dispersed throughout wooded terrain to simulate prone, standing, or sheltered troops. VT-fuzed howitzer projectiles nearing the ends of their trajectories and triggered by their ground-reflected signal were detonated some fifty feet in the air, hurtling their fragments against the improvised soldiers. The lethal effects of each shell burst could be accurately appraised by checking the number of boards that had been struck, and the number of kills thus estimated. Since a high burst sprays fragments over a larger area and a low burst gives increased damage over a smaller area, analysis of Fort Bragg tests allowed the optimum burst height to be calculated. In practice this is achieved by the proper adjustment of fuze sensitivity during manufacture. An artillery barrage of VT-fuzed shells thus transforms the target area into a veritable shower of razor-sharp shell fragments against which neither foxhole, trench, revetment, nor woods offers any protection.

The great advantage of properly placed air bursts had long been recognized by Artillery. With time-fuzed fire, relatively few bursts could be so placed and the element of surprise was entirely lacking, owing to the many shots needed to get on target. A barrage of VT-fuzed shells can come through dark of night, through rain and fog with complete surprise — there is no warning and no shelter. At a laboratory meeting when the engineering of the howitzer fuze was fully detailed, one of

the leading technical men exclaimed that this application was so cruel that he did not think he would be able to sleep nights. His feeling was shared by those who preferred to look upon the VT-fuze as a technological achievement, rather than as a killer. Others, thinking in terms of American lives saved, welcomed this deadly new weapon.

Shortly before Christmas, 1944, Von Rundstedt made his bold attempt to crush the American First Army in the Ardennes. His immediate objective was to cut the Eupen-Liége road, the lifeline of the First Army. Under cover of heavy winter weather and moving his supply trains stealthily at night toward what he thought was certain success, he was met with the first barrage of VT fire which broke over his troops, as if from nowhere. Eyewitness accounts of torn bodies which littered the barrage area fully verified the calculations of destruction forecast by the Fort Bragg tests. To the VT shell, wooden planks and human flesh were the same. What came as a surprise to Von Rundstedt was a carefully planned and executed maneuver. When on October 25, 1944, the Combined Chiefs of Staff gave their approval to prepare for release of the VT-fuze over land, extensive machinery was set in motion to distribute to all theaters of war in Europe adequate supplies of VT material which had been stockpiled from production lines which had been rolling at ever-increased pace since the fall of 1943. Millions of fuzes were ready. During the late fall, 1944, teams of officers trained at Silver Spring and sometimes termed "VT missionaries," under the direction of Colonel H. S. Morton, Captain Jean P. Teas, and E. D. McAlister, had visited all ETO fronts and carefully instructed artillerymen in the potentialities and proper use of VT material. The VT-fuze was introduced to the enemy as a truly secret weapon. Interrogation of prisoners of war established that the enemy had no inkling of its existence and, indeed, even after the VT-fuze had been committed to battle the astounding fact was revealed that the German High Command never did know what was used against them.

After the collapse of the Bulge offensive, and Von Rundstedt's hopes, the VT-fuze continued to be widely used against personnel and was particularly useful in interdictory fire.—Use of key bridges and road intersections was completely denied the enemy by VT interdiction, leaving the targets intact for later Allied use. A report cabled from General Eisenhower's headquarters to the War Department on January 17, 1945,

reads: "According to our observers, the timely release of VT artillery fuzes has vastly multiplied the lethal effect of interdictory and harassing fire. By the unprecedented effectiveness of unseen fire at all hours of day and night, the enemy has been severely upset, as confirmed by prisoner of war reports."

CHAPTER XVII

GUN DIRECTORS FOR THE FUZE

THE VT-FUZE introduced a new variable into the general fire-control problem. VT-fuzed shells must be in the proximity of the target in order to operate; if this condition is satisfied, they result in lethal bursts — the third dimension in the fuze problem being automatically eliminated.

The practice in the Fleet had been to have a large number of 5-inch guns, the principal heavy AA armament, controlled by a master director, the Mark 37, the standard director for these guns. Aside from the fact that the heavy Mark 37 (of which only a few can be installed on any given ship) was especially designed for surface and long-range firing, and is not adaptable for close-in, rapid-maneuver fighting, the control of a bank of guns by this single director resulted either in firing many VT shells with no visible results except on the far horizon (where the fuzes operated on proximity to water), or in grossly "overkilling" one target while being unable to engage other airplanes coming in for a kill.

As a result of observations during the *Cleveland* test of VT-fuzes, a group at Section T (Mindlin, R. B. Roberts, and Tuve) undertook a study of the new factors which the VT-fuze introduced into the over-all fire-control problem, including a survey of the existing directors in the light of these new elements. It soon became evident that there was an urgent need, amounting to an emergency, for smaller, local-control, blind-firing directors in the Fleet. Japanese tactics were changing. Instead of single-plane attacks, the enemy had become wise and was sending attacking planes in groups. If individual gun turrets were equipped with simplified directors, it would permit individual guns to operate a greater portion of the action and engage more targets, greatly increasing the number of kills.

At the direct request of the Navy, Section T undertook a crash program early in 1943 to meet this particular Navy need. Because of the familiarity of Section T with this immediate operational problem in

terms of the properties of VT-fuzes, and the responsibility of that group to follow through from research to production and final battle use, the Director of OSRD decided that Section T should undertake this special program in fire control, even though the general fire-control problem was the province of another part of his organization.[1] It was considered a direct responsibility of Section T to do everything possible to insure that the full potentialities of the VT-fuze would be utilized in the Fleet.

Therefore, in the spring of 1943 a group headed by R. B. Brode, and later (August) by R. B. Roberts, began to work actively on fire control. The early work of the Section in this field was devoted to assisting C. S. Draper at M.I.T. to complete the director system Mark 52, which he had begun. Seven men were sent to his laboratory and worked with him on design problems, while the Eastman Kodak Company initiated production of experimental prototypes. This activity gave Section T its preliminary experience in fire-control problems, speeded up the development of the Mark 52, and produced 15 useful directors which were distributed in the United States and British navies.

However, the Mark 52 was only a temporary solution, as it could not be pointed by radar. Work on the Mark 57 system, which remedied this deficiency, was begun at Section T in August 1943. By November 1943, firing tests were made at Dam Neck on a laboratory model, and a production design was initiated. The first models of this design were tested at Dam Neck during the spring of 1944, and 12 complete units were available for shipboard installation by late summer. Since it usually takes at least two years before a development design of such a complicated piece of equipment is available even in prototype form for shipboard test, the short time scale outlined above well illustrates the speed at which the Section T group worked, in order to meet an emergency situation which they had indirectly helped to create.

The complete Mark 57 director system, as distinguished from the director itself which refers only to the sight box and stand, comprised a Mark 29 (later Mark 39) radar system, an electrical computer and "angle solver" Mark 16, computer Mark 17, and amplifier Mark 1. This system is manually operated and can be used with optical or radar sighting.

[1] This relationship has already been discussed in Chapter IX.

The gyro-computing sight boxes (Mark 17) and the manually operated director stand were manufactured at the Eastman Kodak Company; the computer Mark 16, which converted the gyro outputs and director position angles into gun orders, and the Mark 1 amplifier, comprising auxiliary equipment for the range and range-rate servo and roll correction systems were manufactured by the Submarine Signal Company, a newcomer to the antiaircraft fire-control field. The radar system was the Western Electric Mark 19, converted to narrow-beam operation by Section T and the Wilmotte Company of Washington, D.C. Later, the Section T-designed Mark 39 radar was produced by the Submarine Signal Company.

In addition to the original development of the Mark 57 system, the principal contributions of Section T were co-ordinating development and production at three companies, expediting procurement of the best available components for all parts of the systems. One, a Dynamic Tester, was designed by Mindlin and built in local shops. It enabled any prescribed aircraft course to be fed into the director system, so that an accurate comparison could be made between the gun orders given by the director and those computed "longhand" from the assumed course and ballistic conditions.

Another was the use of two photographic techniques in field testing. One, a stereoscopic testing technique, involves taking synchronized pictures of tracer streams, determining the frame in which each tracer was at the same slant range as the target, and measuring the magnitude of the miss. A second technique, a modification of a method devised by the Naval Research Laboratory, involves the comparison of actual gun orders with orders required to hit the target. The spherical coordinates of the target's immediate space position are given by the director synchros and radar range, and recorded by synchronized cameras which also record director gun orders. Flight time, determined from ballistic data, completes the data required for measuring the accuracy of the director without the necessity for firing a single round of ammunition.

Installations of the first models resulting from this emergency program were supervised by officer teams of Section T men who were put into uniform for this purpose. The crash nature of the Mark 57 program cannot be overemphasized; it was the motivating force be-

hind the program, and of course, the chief reason why the Navy requested that the Section enter this field.

Illustrative of the follow-through spirit with which Tuve had imbued Section T men is the trivial incident which occurred when it was learned that the *Alaska* would be in Chesapeake Bay for a very limited time, and the Section was instructed to make a hasty installation on board. A Mark 57 system was put through expedited final tests in the laboratory and taken to a naval establishment along the Bay. The sea was so rough, however, that the boat crew assigned to routine duty did not see its way clear to transporting the Mark 57 out to the *Alaska*. Whereupon the laboratory man assigned to accompany the director, acting on his own initiative, hired a tug to make the transfer. The *Alaska* got her director. One of Tuve's running orders for the Section was: "I don't want any d——n fool in this laboratory to save money. I want him to save *time*."

Another laboratory rule was: "We don't want the best unit, we want the *first* one." The first local-control, blind-firing director to be installed was a Mark 57, and significantly enough, on the historic *Missouri*. It was one of the four Mark 57's installed on the *Missouri* just before she left the United States, and came directly from the laboratory at Silver Spring where much of the last-minute modification and hand finishing was done.

The personnel-training problem aboard ship was acute — out of more than seventy men trained on board by Section T men, only a dozen had had any previous fire-control training. There was also the natural opposition to working with a new and untried gadget to be overcome. Sixty days of an intensive training period yielded good results, however. Five weeks later, the *Missouri's* first kamikaze plane was downed by fire directed by a Mark 57 director.

CHAPTER XVIII

EXPLODING TORPEDOES BY PROXIMITY DEVICES

THE EXPLOSION of a torpedo in the close vicinity of a target ship not only increases the effective target area, but is often more lethal. It is generally conceded that an explosion occurring under the hull usually "breaks the back" of a ship.

Devices to bring about this lethal result were developed many years ago, but were approached with great caution by the Navy. Torpedoes are extremely complicated and expensive, and only a limited number can be carried on a mission. Should one torpedo premature, it is an effective warning to the target ship and conveniently locates the submarine for counterattack, thus seriously handicapping the submarine's mission.

Operating on a simple magnetic principle, any unevenness in the motion of the torpedo, such as yaw, pitch, or roll, often resulted in premature detonation. Adaptation of such an exploder device to aerial torpedoes (Mark 13), which are dropped at heights of 500 or more feet from torpedo-bomber planes doing 300 knots, was therefore out of the question, even if sufficiently rugged component parts had been available.

Early in 1943, Section T was requested to examine the torpedo-exploder problem from the standpoint of improving existing devices. The Section report indicated that such devices were subject to basic limitations, and that operation essentially free from prematuring could not be expected.

This led to several of the technical men at the Applied Physics Laboratory becoming interested in the challenge presented by the problem of developing a proximity device for aerial torpedoes. If that problem could be solved, the application to submarine warfare would be relatively simple. Aerial torpedoes were often wayward, frequently leaping in and out of the water like berserk porpoises. One leaped directly over

J. E. Henderson as he was attempting to photograph its drop and trail in the water.

Henderson, Director of the Applied Physics Laboratory, University of Washington at Seattle, headed the Section T work on the Mark 9 torpedo exploder. He had been associated with Section T since 1940, during the first investigations on influence devices. Tuve requested that Henderson and his staff undertake the problem of influence torpedo exploders, in line with the policy of encouraging strong technical groups to take part in the general program of the Section.

Also consistent with T policy was the formation at the Silver Spring laboratory of a parallel group to work with the University of Washington team so that intelligent liaison with and direction from the Central Laboratory could be maintained. Tuve held that informed and intelligent direction could be maintained only by having the directors "get their hands dirty"; — failure to do this would result in having the "know-how" remain with the active technical workers, leaving the Central Laboratory as an administrative group only. This policy was, it should be emphasized, universally adopted by Section T and its mention at this point should not be construed as applying with special emphasis to the Seattle contract. Even so, it was important because of the physical distance separating the contractor and the Central Laboratory.

Certain security restrictions with respect to the Mark 9 are still in force. However, it can be stated that an exploder for aerial as well as submarine torpedoes has been developed which is relatively free from premature action, and operates on a totally different principle from the earlier influence devices. Rugged parts similar to those in the VT-fuze were used, particularly for the battery and amplifier, since extremely faint signals must be amplified. The Mark 9 is not affected by roll, pitch, yaw, or broaching, all of which are particularly pronounced when the drop-entry of the torpedo into the water is poor.

The Mark 9 had a difficult development history, with both the Seattle and Silver Spring groups taking the lead from time to time. Its manufacture had to be done along custom-built lines. Parts selection was very critical — many were rejected to the few chosen. A 99 per cent score was demanded and obtained, but not without many discouragements. "The difference between a 95 per cent score and a 99 per

cent score when it comes to torpedo exploders is truly enormous," W. H. Barkas of the Seattle Laboratory once remarked. Much of the early development work, and the pilot Navy production of the electronic parts, was carried out by the highly adaptable Erwood Company of Chicago. Navy production of Mark 9 housings and Mark 13 warheads was done at the Amertorp Company, Forest Park Station, Illinois, and in part by the International Harvester Company.

Produced in some quantity and very extensively tested, the Mark 9 was not proved-in for use until a short time before the cessation of hostilities between Japan and the United States. Thus, in terms of the Section T measure of success — successful use in battle, in time, and in quantity — the Mark 9 torpedo exploder must be regarded as a failure. As a properly operating unit, manufacturable on a factory line and fulfilling difficult technical requirements heretofore unattained, it is a success.

The prove-in tests of the Mark 9 provided a spectacular finish to a hectic development and production program. The MS *Ampetco* was used as the target ship in tests held at Aruba, an island off the coast of Venezuela. In these tests four aerial torpedoes were dropped; the first, a total miss, sank in deep water and no explosion occurred. The second shot, set to fire up to eight feet under the keel, was an influence hit; the effects of this shot were so damaging that the ship began to settle and could not be towed to port for damage analysis. Two more aerial torpedoes were released, five seconds apart (800 feet) on the same course; both shots were under-bottom proximity hits, aft of amidships, and caused the ship to sink in ten minutes.

CHAPTER XIX

THE NAVY CARRIES ON THE SECTION T PATTERN

BY MIDYEAR 1944, the Section T program at the Applied Physics Laboratory had reached full stride. The fuze had arrived at an advanced stage in its development, a so-called universal model which, with minor variations, was adaptable to a wide variety of guns and tactical uses. A good share of the technical effort in the central laboratory could now be turned toward other crash programs, such as the Marks 57, 61, and 62 fire-control directors, the Mark 9 torpedo exploder, and several minor ordnance emergency problems.

Important contributions of a research group are not necessarily confined to technical areas alone. Sometimes, in the long view, perhaps the most important contribution can be a method, a way of thinking, a pattern of operation.

The war highlighted and changed many outmoded ways of doing things, and generally accelerated activity in nearly all phases of national life. In the technological war efforts there occurred a splendid co-operation between the nation's scientific and industrial potential and the military services. In many war programs of which Section T was one example, the welding of science, industry, and the military might indeed be considered a secret weapon in itself.

Each of these three groups surrendered some of their traditional sovereignty in order that the combined team might accomplish the scientific and production miracles which are now a matter of record. The scientist relinquished some of his opposition to regimentation, industry some of its compartmentalization, and the military some of its customary prerogatives. All pitched in together. One might speak of this teamwork in a free society, when its chosen way of life is in jeopardy, as perhaps our greatest secret weapon, yet secret only in the sense that algebra is secret to a Hottentot. It was a prime factor in winning the war.

Using the VT-fuze as an example, Germany tried to develop a similar

device as early as 1930 — ten years before America started on this problem. Indeed, spies sent to this country had high on their priority list securing of any information about possible American attempts at such devices. Colonel C. H. M. Roberts, Army Ordnance, has stated in a report: "Post VE Day investigations by Allied Intelligence Personnel revealed the fact that important contributing causes to the German failure were the multiplicity of types and of technical groups working on them, the complete lack of co-ordination between those groups, and the lack of cooperation, support and direction by German military authorities. This complete disorganization of effort, so unlike the conventional conception of German organization, systematic approach and exhaustive investigation of a subject, was responsible for the supposedly invincible German Army being unable to use the proximity fuze, which in the hands of the Allied Forces contributed so powerfully to the successful culmination of the war."

Section T used to the fullest extent the co-operation between science, industry, and the military in developing a pattern of "follow-through" from research and development in the laboratory, through the production line, and even to the final battle use, as in the case of uniformed "VT missionaries." The availability of the OSRD form of contract made it possible to take full advantage of outside technical and industrial contributions to the program. Fluid organization in the Applied Physics Laboratory was the keynote to rapid progress as was the absence of the so-called traditional scientist's attitude of "here is the laboratory model — my responsibility is ended." As undoubtedly occurred in many other parts of the scientific war effort, nationally known scientists were not above handling a soldering iron, or advising on technical matters on a production line. At Section T, a man might be a project or group supervisor one month, and the next, one of those supervised on another project. A maxim was, "If you're any good, you'll work yourself through — and out of — a job."

Personalities entered, as they always do, as evidenced by the note left attached to a tube-testing machine by a prominent physicist who had not absorbed the full spirit of the immediate job! He had been assigned to the routine, monotonous, but important job of testing one tube after another for microphonics.

The note read:

"If M. A. Tuve's so g—— d—— tough,
Let him test tubes, I've had enough!"

The spirit of teamwork, playing fast technical ball, cutting across lots, follow-through — all with an amazing relative absence of red tape (garden variety) — was considered so important, and the need for preservation of this pattern of operation into postwar years as *sine qua non* for effective national defense, that when Bush pointed out in September 1944 that OSRD, as a war-emergency organization, would be expected to terminate soon after the end of the war, this statement was received with concern within the APL central laboratory and by the Bureau of Ordnance. The Bureau had come to look upon Section T as a civilian arm on which it could lean "extra-curricularly" as a group of scientific and engineering consultants undertaking broad assignments from the Navy and carrying out these assignments by the most applicable technical methods. It was thus free of the traditional limitations of the extensive and closely interlocking organizational setup under which the Navy, as well as other branches of the Government, because of its vast departmental array, necessarily operated. Through arrangement with OSRD, the Navy had allocated certain funds to OSRD for the operation of the Section T program; the central laboratory of Section T at the APL undertook to carry out technical work on problems presented by the Navy and with the approval of Bush. The technical direction of such work, and the manner of its execution (involving the use and technical direction of associated contractors) rested with the Chairman of Section T, as authorized representative of the contracting Government agency, OSRD. Captain C. L. Tyler, Director of the Research and Development Division, BuOrd, served as Special Assistant to Bush from November 1942 until early in 1945. He gave invaluable personal as well as official support to the scientific men at the APL as a result of his knowledge of naval requirements and procedures, as well as his understanding and appreciation of the technical problems involved in the Section T program.

It was in an attempt to preserve the essential features of this workable scheme that in October 1944, Tuve, with the full approval of Admiral Blandy, Captain Tyler, and the Johns Hopkins University, set down on paper what is now known as the Section T Pattern. For the specific

perpetuation of this pattern, a special form of Navy research contract was prepared whereby contracts may be made with independent organizations outside the Navy for work in broad technical areas with a minimum of restriction.

On December 1, 1944, the Johns Hopkins Applied Physics Laboratory entered into a direct contract of this type with the Bureau of Ordnance, Navy Department. Sixteen university and industrial contractors engaged in the general Section T, OSRD, program were also transferred to the new Navy-type contracts, with the understanding that the Applied Physics Laboratory would continue technical direction of these associated contractors.

The Navy Bureau of Ordnance, under the terms of this new type of contract, assigned "Broad Tasks," or areas of investigation. Within this area, however, the technical activities were under the guidance of the Director of the central laboratory. Thus civilian scientists could feel they were an integral part of the Navy, at work on significant problems of national defense, without at the same time being restricted by Civil Service regulations, and the fancied or real intimidation of rank and uniform. The Navy, on the other hand, had a technical team outside its immediate, detailed jurisdiction which could work on broad assignments — a flexible, elastic medium for attack on new and urgent problems, particularly those of a crash nature.

The reader may well feel that despite the seeming freedom of action under this type of contract, a contractor is still rigidly subject to the well-known adage regarding those who hold the purse strings. The fiscal structure is, however, the mainstay of this type of contract. Fiscal responsibility at the Central Laboratory was spread through the organization; each unit, group, and project supervisor had certain sums available to him immediately, on his signature. The delays and friction which result in an organization when one cannot purchase simple laboratory equipment or office supplies without a requisition full of signatures were thus avoided.

In fiscal matters involving larger sums, a pre-approval system is used. This involves the Naval Ordnance Officer (NOO) stationed at the laboratory, who has the power to authorize these expenditures. Very frequently, this was a mere formality; the informal liaison with the Bureau of Ordnance at various levels was excellent, so that as

real technical needs arose, the Bureau was already well informed. The support of the Bureau was splendid throughout; in emergencies and when technical gambles had to be taken, which was not infrequent, the Navy backed the laboratory; the laboratory in turn was ready to accept major technical problems proposed by the Bureau that came within its sphere and for which its technical staff was qualified.

There is also a Navy Ordnance Officer at each of the contractors associated with Section T. (Although the laboratory was no longer under contract with OSRD, the term "Section T" persisted. The transition from OSRD to the Navy was so smooth that many of the laboratory personnel not concerned with administrative matters were totally unaware that any change had taken place!) Similar fiscal arrangements apply to other contractors. If fiscal questions arise which the NOO feels require support at higher levels, the matter is handled through the Naval Ordnance Officer at the Applied Physics Laboratory and almost always settled there, thus by-passing a labyrinth of higher but often less-informed channels.

The Section T type of contract is thus admirably suited to getting on with the job. It has proved to be a means whereby university and industrial laboratories can co-operate with the Services without fear of military regimentation, and conversely, the Services can obtain the most competent scientific and technical research and development work in an extremely expeditious manner.

When carried out in the full framework of the Section T pattern, which involves not only the close co-operation of military officers and civilians, but also provides a channel for the co-operative mobilization of the country's great industrial potential, it has resulted in a highly effective team.

One has only to look at the record to see that it was this welding of science, industry, and the military which produced the tidal wave of technical marvels, in time, and in sufficient quantity to engulf our enemies. Our enemies also had scientific ability; they had industrial capacity; they had a military machine — consider the vaunted German scientist and the much-feared Wehrmacht. Yet, with their political philosophy, a free, co-operative, unregimented welding of these factors was impossible — they could not and did not realize the tremendous advantage in the concept of the team.

We, in peacetime, should not forget it either.

In the months just preceding V-J Day (there was no slackening in the work of "T" after V-E Day) the Central Laboratory at Silver Spring was the nerve center of a vast, country-wide activity. Five major plants were rolling out some 70,000 VT-fuzes a day—millions of tiny radio sets designed to "play" for a few moments! Feeding these assembly plants (Crosley, Sylvania, RCA, Eastman Kodak, McQuay-Norris) were a host of more than 2000 interlocking suppliers and subsuppliers. The Sylvania company, with 23 plants in the eastern part of the United States, supplied over 400,000 tubes per day; the National Carbon Company, with major plant at Winston-Salem, North Carolina, constructed especially for T, and the Eastman Kodak and Hoover Companies were turning out one-tenth of a million batteries a day. A day's supply of plates, which form the principal structure of the batteries, would have made a stack over a mile high, if placed one on top of the other without spacers.

It is reported that 75 per cent of the plastics-molding facilities in the United States were engaged in supplying the various plastic components for the fuze. The nose alone required some 20,000 pounds of ethyl cellulose a day. Several suppliers contributed condensers and resistors at the rate of considerably over 1,000,000 a day representing, incidentally, better than 1½ times the total normal peacetime output of these items. The International Resistor Company furnished the over-all resistor requirements while some 23 companies were engaged in making condensers for the project. Two major companies, Jefferson Electric and Hoover, furnished 170,000 of the tricky mercury switches per day. Allowing 1⅓ grams of mercury per switch, this called for ¼ ton of triply distilled mercury daily. All in all, the VT-fuze program under Navy production contracts involved the production of some 5,000,000 individual components or a grand total of nearly 40,000,000 individual parts daily.

This was indeed a different picture from that of the morning in the late summer of 1940, less than five years previously, when Tuve, Roberts, and Hafstad fired the first miniature tube from their homemade gun. Standing out in the field where the projectile was to land, and holding boards over their heads as a gesture of protection as it fell, each pointed

to the place of fall and thus performed the first rite of the now fully established technique of vertical recovery.

This complex intermeshing of industrial techniques to maintain the large daily flow of radio proximity fuzes was by far the major part of the industrial activity associated with T, though the Mark 57 fire-control director was at this period also in production, and the Mark 9 torpedo exploder had just gone into production. The Navy handled all fuze production, whether the material was for the United States Army, British Navy or Army use. Navy production was co-ordinated and directed by Lieutenant (later Lieutenant Commander) F. P. Schoettle and his staff, on whom fell the considerable responsibility of maintaining a constant flow to the final assembly lines of the individual fuze components. In wartime, with the scarcity of materials and rapidly expanding programs, this was a major task. It was in the Navy Production Division that the battle of the bottlenecks was fought.

The Navy Broad Task assignment to the Applied Physics Laboratory relating to fuzes was twofold; of immediate importance was the maintenance of close surveillance over quality control throughout the production program, and the technical direction of development and production. It was at the request of the Navy that the technical staff of Section T, who were thoroughly acquainted with intricate problems of VT-fuze techniques, originally undertook to guide and expedite this vast production venture. The other part of the assignment was the constant emphasis on the improvement of existing fuzes and the design of new models for adaptation to projectiles for many types of guns. Hence, under one roof, one could find the entire range of scientific technology from basic research to production-line quality control. If one thinks of this diagrammatically, one can imagine a circular band to be entered at any portion of the circumference. If we choose to enter the wheel at the pilot-production segment, we see the many OSRD (later Navy) development contracts in operation; each company has a technical representative at the Central Laboratory, and the Central Laboratory in most cases has a technical representative at the contractor to guide pilot and regular production. There is constant flow of information to this intermediary production staff from the research and development segment at the laboratory. In turn, this leads to full-scale produc-

tion planned and organized by the Navy, but technically directed by the quality-control segment of the Section T wheel.

With such astronomical numbers involved in the production of components, statistical techniques in sampling were developed for this simultaneous cross-checking of several variables and were used in life tests, postmortem examinations, electrical and mechanical testing of rugged tubes, and in the firing of daily production samples at the field. The techniques of statistics played a particularly important part in the successful development and manufacture of the fuze, both as to the completed fuze and its essential components (tubes, batteries, and rear fittings). The opportunities for applications of statistical methods were many and varied. Acceptance of statistical methods was based on standard sampling theory. Due to the destructive nature of much of the testing, the samples were of necessity small, and the control chart became a vital tool in achieving and maintaining acceptable quality levels.

The Central Laboratory through its quality-control groups furnished information, guidance, and encouragement to the manufacturers in the introduction of statistical quality-control methods for inspection and in-process control. General quality-control meetings were held at which there was an exchange among manufacturers of useful and successful techniques in practice. This was one of the best examples of co-operation by all interested groups.

In the tube program, particularly where hundreds of thousands of tubes were produced daily, statistical analysis of all experimental data proved to be a very strong tool indeed in the engineering and acceptance tests as well as in routine quality control. Tests were designed statistically to give maximum precision with minimum cost. It enabled a much closer control to be exercised over the quality of the product and at the same time reduced the work necessary to maintain this quality. The importance of this factor cannot be overestimated. It probably saved several million dollars in the tube project alone.

Quality-control charts were kept on nearly all components and trends were carefully gauged. Trends were especially important with production rates at a high level. By the time a given sample was field-tested, postmortemed, and analysis completed, the production line had rolled merrily on, perhaps by many thousands of units. Frequent telephone communication from the Central Laboratory to all of the assembly lines

and component producers was highly essential to keep this gangling organism moving and functioning. The test-field segment of the quality-control work involved recovery testing at Newtown Neck, over-all service-operability testing at Dahlgren, Fort Miles, Aberdeen, and pattern testing at the University of New Mexico.

The height of the program occurred, of course, after Section T had been formally transferred to the new type of contract with the Bureau of Ordnance. It would be academic, however, to state that Section T ceased its existence on December 1, 1944. The pattern of operation — acceptance of responsibility for the total job of research and development through pilot and full-scale production with close liaison with the Navy Department, and with both Services in the areas of action, was conceived under NDRC and maintained throughout the war.

In line with the spirit of co-operative teamwork the Army gave freely of its facilities and materially assisted the successful outcome of the project. This included the use of the Aberdeen Proving Ground, Ft. Miles, Jefferson Proving Ground, Camp Davis, Ft. Bragg, and others. Furthermore the Army VT detachment at the central laboratory at Silver Spring trained the officers and men of all the Services, who later were sent to the field and instructed the troops in the use of the VT-fuze.

Any history of Section T would be inaccurate without the climax, even though this did not occur until the Section was no longer formally a part of OSRD. The miracle of production could not have been achieved without this basic pattern of close, elastic co-operation and liaison between scientists, engineers, technicians, and administrative talent, the various echelons of the Services, and the great American industrial field.

CHAPTER XX[1]

THE EARLY WORK OF SECTION E

AS HAS been indicated in Chapter X, in November 1940 the National Bureau of Standards joined the group of agencies working on Division A problems. NDRC and the Bureau of the Budget approved the transfer of NDRC funds to this other Government agency in support of the program to be carried out under arrangements made by Tolman and Lauritsen with Lyman J. Briggs, Director of the National Bureau of Standards. In this agreement Alexander Ellett of the State University of Iowa was designated Division A Liaison Officer with the Bureau of Standards. A month later, Section E of Division A was set up with Ellett as Chairman. The project at the National Bureau of Standards was assigned to its supervision. These steps formalized the co-operation already furnished by Bureau scientists to a number of NDRC groups.

A few months later, H. L. Dryden, Chief of the Mechanics and Heat Division of the NBS, was appointed Vice-Chairman of Section E. W. D. Coolidge, Director of Research, General Electric Company, and Briggs were appointed members of the Section in September 1941.

Originally, Section E was created not wholly as a fuze-development group, but rather as an organization which might render valuable services to other NDRC groups and in particular to other Sections in Division A, such as Section H, the rocket-development group, and Section T, Tuve's group. However, by 1943, it had grown from this type of organization to a major division of the NDRC, handling all bomb, rocket, and mortar proximity fuze-development with a budget of some $6,000,000 per year, with contracts placed with twenty or more commercial and academic institutions.

During the first year and a half, the Section's activities were varied. While principal effort was directed toward fuze development, effort

[1] Some of the material in Chapters XX to XXIII, inclusive, has already appeared in a pamphlet entitled "The Radio Proximity Fuzes for Bombs, Rockets, and Mortars," prepared in October 1945 by the senior staff of the Ordnance Development Division of the National Bureau of Standards.

also went into other channels. Among the first projects completed by the Section was the development of a machine for rapidly extrapolating to greater depths the magnetic field of a degaussed ship from observed values of the vertical component of its field at one fixed depth. Other projects under Section E's direction were (1) development of guided missiles, (2) development of an acoustic proximity fuze, (3) development of radio equipment for transmitting data on the behavior, under actual flight conditions, of proximity fuzes mounted in bombs and rockets, (4) development, for use in proximity fuzes, of a compact battery having good low-temperature operating characteristics, (5) development of apparatus for measuring and recording explosion pressures and thrust developed by rockets, and (6) development of equipment for detection of loose driving bands on shells.

However, by July 1941, the development of proximity fuzes was taking precedence over minor projects. In a memorandum of July 11, Tolman formally assigned responsibility for all bomb and rocket fuzes to Section E. The photoelectric bomb fuze group which had been working at DTM moved to the National Bureau of Standards, where another group was already active on this problem. Generally speaking, Section E's principal job became the design of proximity fuzes, both radio and photoelectric, for all nonrotating or fin-stabilized projectiles: bombs, rockets, and trench mortars. This division of responsibility made for faster progress and less duplication of effort, but from this time on the liaison between Section E and Section T was much reduced.

Work on a radio fuze had started at the Bureau in the latter part of December 1940, in close co-operation with the Section T group on the same project at the Department of Terrestrial Magnetism. Ellett recognized that the broad experience which Harry Diamond and W. S. Hinman, Jr., both of the Radio Section, Electrical Division, of the Bureau, had gained in the design of miniature-radio equipment in connection with their development of radiosondes, would prove invaluable in the design of miniature electronic fuzes. Briggs agreed to make these men available.

The Navy was experimenting with a jet-accelerated armor-piercing bomb and was anxious to obtain a fuze to ignite the rocket part of the bomb automatically as it approached within several hundred feet of its target. During the first few months little thought was given to specific

applications; effort was directed toward devising workable circuits. It was soon apparent that a fuze utilizing the Doppler effect of reflected radio waves was the most promising. Diamond and Hinman devised a diode detector arrangement whereby any reflected radio wave would cause variation of the radio frequency currents in an antenna which would produce the desired signals. These signals were amplified so as to trigger a thyratron, setting off an electric detonator and exploding the bomb.

The next problem was to determine the nature of the reflected signals to see if such a detector would be practical. Using acorn tubes and radiosonde batteries, a simple model was made up; this consisted of a square box with radio tubes and batteries properly arranged, with rods projecting from each end as an antenna. Its operation was tested by raising and lowering the model over ground by ropes and pulleys. Signals reflected from the ground were measured and found to be strong enough to promise successful operation. The fuze was well along to becoming a reality.

But how would the circuit respond to the passage of an airplane? Could it be used in an antiaircraft fuze? What type of amplifier would be necessary? What would the value of the circuit components have to be? These questions were partially answered when the model was taken to the National Bureau of Standards Field Station at Camp Springs, Maryland. A private pilot, who was not informed of the nature of the experiment, flew over the model, fixed at the top of an 84-foot radio tower. When the plane passed within some 50 feet of the model, indication of its passage was observed and the diode detector circuit had proved itself. Numerous other tests of this nature were later conducted at the Naval Proving Ground at Dahlgren, Virginia.

Perhaps the most informative of these tests were those conducted at the Naval Air Station at Lakehurst, New Jersey, by R. D. Huntoon. Going aloft in a Navy blimp with recording apparatus, he suspended fuze models beneath. While fighter planes dived past the models, Huntoon photographed oscilloscope traces of the form of the signal received by the fuze as the plane passed it. All of this information later was to prove invaluable in the design of fuzes for antiaircraft use.

Another ingenious test was carried out at the same time at Camp

Springs. Fuze models were carried aloft by large meteorological balloons (tethered) and the balloons shot down with a .22-caliber rifle. After several failures, a model was released at an altitude of about 300 feet and functioned by detonating a test charge at about 40 feet above ground.

Three models were built for preliminary operational trials. These were mounted on practice bombs and were dropped at the Naval Proving Ground at Dahlgren from a plane flying at 3000 feet. Two of these models operated after about eight seconds of fall (the time to arming); the third functioned properly about ten feet above the water. A second set of three models was then constructed and tested in the same manner. Similar results were obtained, one functioning at about ten feet above water.

New models were made, six of which were tested at Dahlgren. With the bomber flying at 3000 feet, the bombs were released separately. All six functioned at heights of 150 to 300 feet over the water, these heights corresponding to the responsiveness of the fuzes as determined in the laboratory. These tests proved conclusively that a radio proximity fuze was practical. It remained to add the detailed engineering and development work needed to turn a "first model effort" into a producible item fitting Service requirements.

While these first models proved the principle practical, they did not provide all of the components and practices required to produce a satisfactory Service fuze. They were, however, based on principles which were adopted for all future radio fuzes. All components were mounted rigidly. Circuit elements were either completely immersed in wax or were tied to a rugged frame structure and given a heavy protective wax coating. Shock mounting was not used; all components and connections were made so stiff and rugged that there was very little relative motion, even under severe mechanical vibration.

The first models were cumbersome. While the bomb body was utilized as part of the antenna, a separate section of about equal length projected out of the tail of the bomb. From an operational standpoint, this added length was objectionable since its use would lessen the total bomb load which could be carried by the bomber. Later circuit developments showed the feasibility of driving across a split near one end of the antenna, and the length added for this purpose was reduced to

about two inches. During this period practically all of the work, including construction of models, conducting of tests, etc., was carried on by Diamond, Hinman, Huntoon, L. S. Taylor, C. H. Page, and Cledo Brunetti, the latter two having been added to the staff in the spring of 1941.

It was now apparent that each fuze application would present a different problem in mechanical and electrical design, since the vehicle upon which the fuze was to be carried served as an antenna, and since arming and safety requirements differed widely. Generally speaking, each tactical situation requires a different fuze. Views of military personnel regarding the relative tactical value of proximity-fuzed bombs or rockets for air-to-air, air-to-ground, or ground-to-ground use differed widely. The possibilities were recognized by very few, and much missionary work had to be done before definite requirements for production of any specific type were set up. Thus from January 1941 to May 1942, the fuze group accumulated data and developed components and mechanical designs with a view to obtaining sufficient flexibility in design so that a fuze could be made for any particular application.

On one point there was general agreement among Service personnel — that fuzes must be designed for use on existing projectiles. While this simplified logistics, it greatly complicated fuze design and introduced many nonessential problems. Had work on an integrated design of bomb and fuze been started early, there is little doubt that a more effective weapon would have resulted. Suggestions along this line were made early, but the Army was not ready for such an innovation and it was only during the closing months of the war that it became interested in this development.

During the early part of 1941 there was much talk of the use of rockets as antiaircraft weapons. The British were already using rockets in the defense of London. Equipped with proximity fuzes, they would indeed be a formidable weapon. In fact, the British had been working since 1938 on the development of photoelectric fuzes for rockets. Encouraged especially by Lauritsen, who had just returned from England, work of the radio fuze group was extended to include the development of rocket fuzes. After transfer of the photoelectric fuze-development group from Section T to Section E at the NBS in July 1941, the

work of this group was directed almost entirely to the development of rocket fuzes.

It looked as if the photoelectric fuze would come through first and its development was pushed vigorously. A photoelectric fuze is just what its name implies — a fuze which is sensitive to changes in light intensity. It is a photoelectric cell equipped with a lens. When an object passes between the lens and the sky the light intensity changes, causing a change in the photocell current. This change, after being amplified, triggers a thyratron which in turn explodes the fuze. Unfortunately, the fuze has two inherent faults: (1) it depends on light for operation and cannot be used at night, and (2) it will fire when the sun moves into and out of the lens. Circuits designed for bomb fuzes were quickly adapted for use in rocket fuzes. First models built at the Bureau were tested on 3¼-inch rockets at Indian Head, and later additional models were tested at Aberdeen August 31.

Performance of the initial models was good, and in September the Bell Telephone Laboratories were asked to develop a production design of a fuze for use on the British rocket and to supply 200 fuzes for test. Design and construction of the rocket fuze proceeded rapidly. By October 25, manufacturing drawings and specifications were complete and the 200 models had been delivered. Although field tests indicated the need of certain modifications, field performance was good and the Army placed an order for 5000 fuzes. However, British interest in this fuze soon waned and the 5000 were not delivered until months later and were never used operationally.

Fuzes for use on fast-burning, as contrasted with the British slow-burning, rockets were also developed. The Bell Telephone Laboratories completed manufacturing specifications for a fuze for the Army's 4½-inch rocket and delivered ten models. These were tested at Aberdeen, with four of the ten fuzes functioning properly.

Work also was underway at the Mansfield plant of Westinghouse Electric and Manufacturing Company in the spring of 1942 on the design of a fuze for the 4½-inch rocket. W. J. Russell's group at Westinghouse worked with the PE fuze group at the Bureau and in general followed closely designs of the Bureau group. When ten of their fuzes were tested, three of them functioned properly. Shortly after procure-

ment of parts for 1000 fuzes had been initiated, all work on this model was suspended, due to its large size. During this same period several experimental lots of fuzes were also constructed at the National Bureau of Standards.

Thus, by May 1942, the technical problems, except for sun firing and night operation, the two basic limitations of the fuze, had been largely solved. Considerable engineering and development were still needed to reduce the fuze in size and weight.

Work on a radio fuze for the British rocket proceeded more slowly. A fuze which fitted between the rocket motor and the high-explosive head was designed. Except for the size of the fuze occasioned by the large commercial batteries, the fuze was reasonably good. Some quantities were tested at Aberdeen against ground and air targets with considerable success, but the development was not processed through production of more than a few lots.

Several lots of both bomb and rocket fuzes were field-tested during the first few months of 1942. Some 75 radio bomb fuzes were dropped during the period from January to May. Of these, 50 functioned satisfactorily, 10 were premature, and 15 were duds. In addition, some 50 rocket fuzes were fired during the same period. Of these, over 90 per cent withstood successfully a 500g acceleration and over 50 per cent functioned satisfactorily against ground. The last lot of rocket fuzes tested gave 70 per cent satisfactory performance.

Although Section E, later Division 4, was primarily concerned with fuze development, the arrangement with the Bureau of Standards was purposely made flexible to provide for other problems which might arise in the general field of Division A. A minor project of this sort was the development of an antiaircraft target rocket. The development of a target rocket was started in the summer of 1941. Discussions between NDRC representatives and General J. A. Green, Chief of Coast Artillery, led to the conclusion that a rocket would be very desirable as a target for training purposes. Such a target rocket would have to have several characteristics: an elevation of 200 to 400 yards, a velocity of 250 to 300 miles per hour, and visibility at a firing range of 500 to 2500 yards.

At the time of initiation of the program, Section E had already developed a crude rocket at the National Bureau of Standards in connec-

tion with work on fuzes. This rocket was ideally suited ballistically for the target application but its visibility was poor. In an attempt to improve the visibility, two methods were tried: (1) large vanes or tails, and (2) smoke pots.

Two rockets with large rectangular, 1-by-2-foot tails, made at the Bureau, were fired at Aberdeen August 31. The visibility was good but the velocity of the rockets was decreased considerably by the addition of the large tails. The original large tail was redesigned by C. C. Lauritsen, who made it semicircular and much lighter. The velocities acquired by the rockets were sufficient and construction was started at the Bureau.

Successful tests were carried out at Fort Monroe on October 11. The velocities obtained were higher than those that are possible with towed targets. The trajectory varied in direction, and the second half of the trajectory simulated a dive bomber. Additional rockets were fired on October 30, at which time it was demonstrated clearly that use of a smoke trail was not of much aid.

The problem of designing a mobile launcher for each training center was assigned to H. B. Bean at the NBS. A satisfactory design was completed and a single model constructed by the Bureau.

To expedite the manufacture of a sizable quantity of target rockets, it was suggested that they be procured through the Bureau which was to be reimbursed by the Ordnance Department. This financial arrangement was agreeable both to Section E and to the NBS, and the Bureau immediately placed orders for 3000 rockets with the Revere Copper and Brass Company, 30-trailer-type projectors with the L and S Welding Company, and 13,000 pounds of powder from the Hercules Powder Company. No further work was done by Section E on the project. Initial trials of the rockets by the Army were very successful and use of them in the training of antiaircraft crews had become standard procedure by the end of 1942. Nearly a million such rockets were used.

In the fall of 1941, Section E assisted the Navy in connection with radar ranging on antiaircraft shell bursts. Captain Lybrand Smith asked NDRC assistance in the development of bursting charge modifications in order to obtain longer duration of radar echoes from antiaircraft shell bursts. It was expected that use of such a charge would be helpful

in development of a radio-reflection method for fire-control adjustment. Equipment was designed and constructed at the NBS under Huntoon for the measurement of the duration of the echoes in order to determine the effects of changes in composition of the bursting charge and in the frequency of the radar. Preliminary tests were conducted by the Bureau, and continued by the Bureau of Ordnance. Section E continued to co-operate on a consulting basis.

Section E also sponsored some work on the development of a continuously adjustable electric time fuze and a barometric fuze.

In discussions with the British in the summer of 1941, C. C. Lauritsen learned of their need for a time fuze for use on their high-altitude anti-aircraft rockets. In August, a contract was placed with General Electric for the development of a continuously adjustable electric time fuze. Specific requirements demanded that the time to function be continuously adjustable from 1 to 20 seconds and that the time-setting device control nine units in parallel, time setting to be changeable by remote control in a few tenths of a second. Temperature range was from 0 degrees to 40 degrees Centigrade. About 100 fuzes were made plus several sets of remote-control equipment. Work on the time fuze was terminated when the proximity fuze was well along.

Development of the barometric fuze was carried out at the University of Chicago, under the supervision of S. K. Allison. Low priority was given the project. Some models were constructed and laboratory-tested, but none dropped from an airplane. As airburst requirements could be met best by the proximity or a manually set time fuze, the project was terminated.

Soon after work on proximity fuzes began, the need for a battery having good operating characteristics at low temperature was apparent. George W. Vinal, Chief of the Section of Electrochemistry at the NBS, headed the investigation. Vinal had found, previous to 1941, that a battery using a particular combination of electrolyte and electrode material had good performance at low temperatures. A reduction in the size of this battery without affecting its performance remained to be accomplished. This involved largely the development of small electrodes which would function properly and which could be produced in large quantities cheaply. By July 1941, 135-volt batteries had been made within dimensions of 2¾ inches in diameter and 2½ inches in height.

Urgency of other work prevented further work on this problem until May 1942, when the National Carbon Company undertook the development of this type of battery to replace the dry battery contemplated for use in the rocket proximity fuzes. The problem was largely one of design. If possible, the electrolyte was to be kept in a closed reservoir until the rocket was fired, with the forces of setback and reverse g being used to break the reservoir and to distribute the electrolyte to each of the cell cavities and to clear short circuits. National Carbon worked nearly two years without developing a satisfactory battery. The small accelerations present in rockets were not sufficient to break electrolytic bridges which caused intermittent shorts. Meanwhile fuzes incorporating generator power supplies were in production and work at National Carbon was concluded.

In addition to work on the battery, considerable work was done both by Vinal's group and by the National Carbon Company on improvement of performance of dry cells at low temperature by the use of special mixes. These studies indicated definite promise of improved low-temperature operation.

During 1940, the Naval Ordnance Laboratory was pushing vigorously problems relating to the protection of ships against the hazard of magnetic mines. In early November, G. Breit of the Naval Ordnance Laboratory discussed with Tolman the need of a device for setting to scale a physical three-dimensional model of the magnetic field of a ship as it exists below the measured plane in which the ship's signature was observed on the range. Some preliminary studies were made by Ellett and DuMond in November and December 1940 while still working at DTM. Forty thousand dollars was set aside to cover the cost of this work which was to include the construction of a single model for delivery to the Navy. The design of the apparatus with its switching devices, cabling, and solenoids resembled closely an automatic telephone exchange, and detailed designs were developed by the Bell Telephone Laboratories working closely with J. W. DuMond, at that time at the Bureau of Standards.

The entire equipment was assembled and tested at the NBS. A formal exhibit of the equipment was made on April 10, 1941, after which the equipment was dismantled and delivered to the Naval Ordnance Laboratory where it was assembled and used extensively.

CHAPTER XXI

ROCKET FUZE PROGRAM

MAY 11, 1942, was a memorable and long-awaited day in the history of Section E. It was then at a formal meeting of the Section that the Army stated its first definite and urgent requirement for a specific fuze. This was a fuze for the Army's new 4½-inch airborne rocket. The Army was relying heavily on this weapon to combat the then all-powerful Luftwaffe. Responsibility for development of the weapon was to be divided three ways: Lieutenant Colonel L. A. Skinner's rocket group in the Ordnance Department was to be responsible for the rockets; the Army Air Forces for aircraft installation; and Section E for the fuzes. Specifications demanded that the fuzes be considerably smaller than any so far tested, displacing inside the high-explosive chamber a cylinder approximately 2¾ inches in diameter by 5 inches long. It was further required that they should function with at least 50 per cent reliability on passing in the neighborhood of a plane.

Section E accepted the challenge and immediately gave this program the highest priority. The time scale was short, but in the twelve weeks allotted complete specifications for both a photoelectric fuze and a radio fuze were proved-in by extensive field testing and delivered to the Ordnance Department. Enthusiasm ran high. Within a few days after the May 11th meeting, general plans for carrying through the development program were agreed upon. Many matters regarding over-all policy had been discussed at an executive session of Section E held in the afternoon of May 11th; other matters had been discussed in informal conferences between representatives of Section E, the Services, and representatives of the several development groups involved. The general plan, as originally outlined, was followed without substantial change and led to successful large-scale production of both radio and photoelectric fuzes by January 1943.

It was decided to push development and design of *both* radio and photoelectric fuzes. The Ordnance Department initially was interested

primarily in the photoelectric fuze as they considered that its development was further along. Limited procurement of 50,000 photoelectric fuzes was approved by the Ordnance Committee on May 7, but the severe limitations in this fuze, plus the promising performance of early models of the radio fuze, were considered by Section E to justify development of the radio fuze on an equally high priority. The Ordnance Department came to agree with this view on June 18, 1942, when the Ordnance Committee approved limited procurement of 50,000 radio fuzes.

Mechanically the radio and photoelectric fuzes were interchangeable. An assembled fuze was made up of four components: nose, battery, safety and arming switch, and housing. The two fuzes differed only in the nose sections. This simplified procurement problems.

Responsibility for the detailed design of each component was assigned to a specific group. Designing the nose of the radio fuze was done by Diamond's group, working closely with engineering groups at the Friez and Westinghouse companies in Baltimore. Henderson's group and the Bell Telephone group worked more independently on designs of the photoelectric fuze.

The development of a suitable small battery was undertaken by the National Carbon Company and the Burgess Battery Company. Tube-development contracts were placed with Raytheon and Sylvania Electric Products, Inc. Raytheon was to improve the performance of tubes already in use at the Bureau and Sylvania was to modify tubes, which it was producing for Section T. This was principally in the direction of de-ruggedizing the tubes so that when so modified they would not compromise the guarded security of the shell fuze.

Work got under way at once. Within two days after May 11, preliminary designs were complete and construction of test-fuze lots began. Various expedients were adopted to rush completion of samples for testing. Complicated mechanical and plastic parts were fabricated by hand to avoid the delay incidental to procuring tools and molds for high-speed production. Temporary switch and safety mechanisms were used. Until battery development was complete, available batteries too large for final use were temporarily utilized. Orders were placed for electrical components sufficient to make several thousand fuzes.

Fortunately a small pilot line was already in operation at the Bureau

of Standards under the very capable supervision of A. S. Clarke, who had joined the fuze group in January 1942. More than 1000 fuzes were built by Clarke's line during June and July.

It was recognized at the outset that a design could not be proved-in without construction of several thousand models; for, while laboratory tests simulating flight conditions were very valuable in development and production, they were never conclusive. Field performance of the fuze was the only reliable measure. Field testing is a "one try" proposition, since each test destroys the fuze. As evidence is obtained on a statistical basis, literally thousands of test rounds are required to establish fuze quality or to examine and determine the cause of some defects.

The serious need for a single group responsible for the field testing of equipment developed by the various development groups had been recognized early in the program. In June 1941, a separate Field Test Group had been set up at the National Bureau of Standards under the direction of L. S. Taylor. Initially, tests were carried out by arranging with the Services or with other NDRC groups for the use of facilities at their proving grounds; later, there were set up proving grounds devoted entirely to Section E's work. Personnel of the field-test group loaded and assembled the fuzes. They obtained and analyzed data pertaining to the performance in the field of the items developed, and promptly reported results. Special equipment, such as radio reporters, recording equipment, special photographic techniques, etc., required to obtain the data wanted, were developed and constructed by them.

There were evident advantages in having a single and detached group. Smoother relations resulted with the Army and Navy proving-ground personnel since they were at all times working with a single group. Because development groups had to formulate test programs in advance, there was better-planned testing with fuller utilization of testing facilities.

From July 1941 to May 1942 the test group was engaged largely in the testing of rockets and rocket fuzes. While a few preliminary rocket tests were made at Section H's range at Indian Head, Maryland, in August 1941, the bulk of the early rocket tests were carried out at Aberdeen. Operations proceeded there fairly smoothly until the United States entered the war. After that, planned operations were virtually

impossible, due to the low priority given by the Army to the fuze program.

The test group developed several special techniques and pieces of equipment needed to obtain requisite data. High-speed motion pictures were taken of all new types of rockets for the purpose of obtaining information on velocities, accelerations, and general firing characteristics. To locate the position of the burst that indicated action of the fuze, the burst was photographed from at least two positions.

A major contribution of the test group was the development of radio reporters. Such a reporter consisted of a radio transmitter that could be mounted on a bomb, rocket, or mortar projectile together with the fuze. It transmitted to ground observers data on the performance of the fuze circuits and mechanisms throughout the flight of the projectile. Reporters made possible the determination of the nature and source of microphonics in both radio and photoelectric fuzes, the determination of the type of pulse imparted to a photoelectric fuze as it approached its target, the speeds of wind-driven generators, etc.

The use of such reporters was proposed by the test group in March of 1941 largely as a result of dissatisfaction at having tested several photoelectric fuzes without obtaining any significant information as to the cause of malfunctions. From that date on, a large number of reporter units were built and used extensively in all phases of the fuze program.

Development and maintenance of suitable aerial targets for testing fuze functioning also was a major problem. A target used extensively for early photoelectric fuze work was a 12-foot spherical black balloon suspended beneath a larger balloon of the simple barrage type. For early work with the radio fuze an antenna array, 40 to 60 feet long, consisting of a series of crossed dipoles, was suspended from the barrage balloon. Both types of target were reasonably satisfactory but introduced extensive maintenance problems and could not be used at all in high winds.

Procurement of several thousand fuzes and their components for testing presented a major problem. Much of the success of the rocket fuze program was due to Ordnance Department co-operation in this connection.

The latter part of May, Tolman informed Brigadier General (later Major General) G. M. Barnes, Ordnance Department, that both the

radio and photoelectric fuze designs could be in suitable shape for quantity production early in August, provided the Army Ordnance Department would co-operate by immediately negotiating and financing such additional development contracts as were needed to prepare the way for quantity production of all components and of complete assemblies of the new compact designs. Colonel H. S. Morton had already paved the way for these contracts and during the early part of June the following were let: National Carbon Company for 3000 batteries; Burgess Battery Company for 2500 batteries; Central Scientific Company for 3000 3¼-inch rockets; Globe-Union Inc. for 5400 safety and arming switches; General Electric Company for 500 radio fuze noses and safety and arming switches; and RCA Manufacturing Company for 2000 photocells.

Other components and fuzes were obtained either from Section E contractors or through Bureau purchase orders. For example, the first 100 photoelectric fuze noses of the Bell Telephone Laboratories' design were made up by them under a Section E contract. The next 1000 were purchased by the Bureau from Western Electric. The initial 500 Westinghouse radio fuze noses were purchased by the Bureau, using early models supplied by Westinghouse under a Section E contract as a basis for procurement specifications.

To facilitate exchange of information and co-ordination of the work of the several agencies, a Standardization Committee composed of representatives from Section E, the Ordnance Department, the pilot-development groups at the National Bureau of Standards, Signal Corps, and the prime commercial contractors, was set up by Tolman. Membership was as follows: Alexander Ellett, Chairman, Section E; J. F. Wentz, Bell Telephone Laboratories; W. J. Russell, Westinghouse Electric and Manufacturing Company (Mansfield); C. J. Julian, Julian P. Friez and Son Company; H. S. Morton, Colonel, Ordnance Department; J. S. Lambert, Captain, Signal Corps; R. J. Biele, General Electric Company; R. N. Harmon, Westinghouse Electric and Manufacturing Company (Baltimore); H. L. White, Globe-Union Inc.; H. Diamond, National Bureau of Standards; J. E. Henderson, NBS; W. B. McLean, NBS; and J. S. Rinehart, Secretary, Section E. Regular weekly meetings of the Standardization Committee were held from the middle of June through August. Several subcommittees were appointed for prepara-

tion of detailed specifications, and rough drafts covering all components and fuzes were prepared during July. By the end of August specifications for all parts of both fuzes were substantially final.

By the first of July several lots both of radio and of photoelectric fuzes had been field tested. Regular deliveries of experimental lots of fuze noses were being made by Westinghouse, Friez, and Bell Telephone. The National Carbon Company was making regular shipments of batteries and it appeared that this would be the only source, since Burgess had been unable to develop a satisfactory battery. Globe-Union had begun shipment of safety and arming switches, and in August Sylvania began to deliver satisfactory tubes. During July and August principal effort was directed toward refinements in designs. During this period some 300 radio fuzes and some 500 photoelectric fuzes were tested. In general the level of performance of all these was above 60 per cent.

Increased necessity for extensive proving-ground facilities made it imperative to find a proof area controlled and operated exclusively by Section E. The inability to schedule tests at Aberdeen caused serious delays. In the spring of 1942, Ellett, Colonel Morton, and Taylor went scouting to find a suitable location.

As a result of this trip, arrangements were made with General J. A. Green and with the Commanding General of Camp Davis for the use of land at Fort Fisher near Wilmington, North Carolina. The land was ideal for testing both rockets and fuzes. It was sandy and flat and permitted excellent observation. One side was bordered by the ocean so that free ranges from 100 yards up to several miles were possible. The Army agreed to assume complete responsibility for security, and to furnish assistance in constructing buildings, roads, etc.

Initial buildings, constructed in May and June by the Army, were a roofed-over balloon hangar, sufficiently large to accommodate a 10,000-cubic-foot barrage balloon, a laboratory building, and a darkroom completely equipped for film processing. There was a bunkhouse, a concrete powder magazine, and a powder-handling and -loading room. In defiance of tides, stabilized roadways were constructed connecting all buildings, firing points, and the hangar. A complete pole line was established for intercommunicating and outside telephones, and the necessary wire for camera and fire-control lines.

During the first few months personnel for operating the proving ground were drawn largely from the Washington group, with senior personnel generally spending alternate weeks at Fort Fisher, although during construction Taylor spent almost full time there, working closely with the Army. By June, Signal Corps and the Ordnance Department had, at the insistence of Taylor, assigned some twelve technical and safety personnel to the operation. In August 1942 an Official Field Station of the National Bureau of Standards was established at Fort Fisher. About twenty subprofessional and junior professional personnel were assigned there permanently.

The testing of fuzes at Fort Fisher started on May 5, 1942. Radio rocket fuzes were fired out to sea for test of their function over water. Photoelectric fuzes were tested against a balloon target. Later radio fuzes were tested against the dipole target described above.

Balloons from which the targets were suspended became bottlenecks. Procurement of the balloons was difficult. Several were lost through breaking of the mooring ropes. High winds frequently caused delay. Field testing with this equipment was a slow and laborious process. Twelve test rounds per day was a good average. In addition, in firing radio fuzes at high angles the reflected wave from the ground was troublesome. Both problems were solved by an ingenious method first suggested by Diamond. The rocket projector was mounted on a 30-foot tower and a target suspended at the same height from poles about 1000 feet away. Initially the target was a sheet of chicken wire about 60 × 10 feet. Later, however, an 0.8-size mock-up of a B-25 airplane consisting of a wooden frame covered with chicken wire was used. The first firings against this target were made July 4th. It worked well and made possible the testing of a fuze per minute.

The small black balloon suspended from a barrage balloon was used until mid-September as a target for the testing of photoelectric fuzes. At that time, a stationary target, consisting of large pieces of black cloth suspended from poles, was set up and thereafter was used almost exclusively.

During the period from July 1 to October 31 the group at Fort Fisher was engaged principally in the testing of experimental radio and photoelectric rocket fuzes and in studying the ballistics of rockets. By the end of June the proving-ground crew was operating smoothly. A review

of the records shows that some 40 to 50 rounds were being fired each day, weather permitting.

Rockets were, of course, essential for the testing of fuzes. Although Colonel Skinner had, at the May 11 meeting, promised 4½-inch rockets for testing purposes, production was delayed and thus rockets were not available in quantity until late fall. Fortunately Section E had a small supply of 3¼-inch rockets and tools and facilities for producing more. This fortunate circumstance had come about as follows: When Section E began the development of proximity fuzes for rockets in June 1941, it was apparent that testing of the fuzes would require substantial numbers of rockets. Few British rockets were available, and only small amounts of rockets were being made in this country. These latter were being used for experimental purposes by the rocket-development groups. To insure an adequate supply of rockets for its own use, Section E began to build them. The initial plan was to design and to produce in small quantities a satisfactory rocket motor to serve as a vehicle for the various rocket fuzes. No thought was given to the design of a rocket for use as a weapon.

T. Lauritsen, working at the National Bureau of Standards, was largely responsible for the design. During July he drew up designs for several 3¼-inch rockets, designed to use the fast-burning powder then in production by the Army. The first models of these were made in the Bureau shops. Later the Bureau placed an order with the Central Scientific Company for 100 rocket motors. Subsequently several other orders were placed with them. The Revere Copper and Brass Company was also producing, in quantity, the 3¼-inch rocket developed by Section E as a target for antiaircraft gunners. By changing the fins on these they could be used for testing rocket fuzes. To relieve the rocket shortage the Ordnance Department, upon Ellett's recommendation, placed an order with Cenco for 3300 rocket motors and ordered 1000 modified target rockets from Revere. These were delivered during July and August. Without them the rocket fuze program would have been delayed weeks and perhaps months.

Complete procurement specifications for the radio proximity fuze and the photoelectric proximity fuze were sent by Ellett to Colonel Morton on September 3, 1942. These specifications were prepared by the Standardization Committee. They permitted the greatest possible

freedom in manufacturing techniques and choice of materials on the part of the manufacturers. Extensive testing of fuzes and of fuze components continued through September. This resulted in many design modifications and minor changes in specifications. By the end of September, it appeared that Section E's job was nearly done and on September 30 Ellett regarded the development finished and relinquished full control of it to the Army. The point was reached when the time lost in incorporating even minor changes outweighed any probable improvement.

Total cost of the program, exclusive of materials supplied by the Ordnance Department, was about $1,250,000.

Responsibility for large-scale procurement of the fuzes was assigned to Signal Corps. It is likely that those who made the decision did not recognize that an electronic fuze is a peculiar item, neither all ordnance nor all electronic. Unfortunately, the electronics industry had no experience with production of ordnance equipment nor was it familiar with really large-scale production methods. On the other hand, normal ordnance equipment suppliers had no experience with electronic equipment. Electronic equipment was foreign to Ordnance Department development and procurement groups, and Signal Corps, normal War Department electronics-procuring agency, had never procured ordnance items. This situation was corrected in 1944 by the assignment of full responsibility for VT-fuze procurement to the Ordnance Department.

During the summer of 1942, Signal Corps began making arrangements for large-scale procurement. In May, the Ordnance Committee had authorized procurement of 50,000 photoelectric fuzes and in June had authorized procurement of an equal number of radio fuzes. In mid-July procurement of an additional 75,000 of each type had been authorized. By September 1st, Signal Corps had placed sizable orders with Westinghouse, Friez, Philco, and General Electric for radio fuze noses; and with Westinghouse, Rudolph Wurlitzer, and Western Electric for photoelectric fuze noses; with National Carbon for batteries; with Globe-Union for safety and arming switches; and with the tube manufacturers for tubes. During September and October, engineers from the Bureau spent much time in familiarizing engineers of the above companies with the designs and with the techniques of assembling and testing fuzes. By November manufacturing drawings were substantially

complete. Production in most cases was scheduled to begin in January.

Although an extensive Signal Corps quality-control laboratory was set up at Belmar, New Jersey, the Signal Corps admitted that it had neither the equipment nor skilled personnel required to operate it. Most of the senior personnel for this group had to be transferred from development groups at the National Bureau of Standards. While this transfer seriously handicapped the Bureau, it was considered essential if fuze procurement was to go forward expeditiously.

Delivery of fuzes began in December and field testing of samples from Signal Corps production lots of both types of fuzes was started at Fort Fisher. Ordnance Department facilities for field testing at Aberdeen were delayed and Signal Corps requested Section E to assist in tests of type-approval samples. By the end of January it was apparent that transfer of testing to the Ordnance Department would be further delayed and Signal Corps requested Ellett to continue assistance in testing of production lots. Ellett agreed to this arrangement, which was again extended in April.

About this time a National Bureau of Standards' Blossom Point Proving Ground in Maryland was set up about 50 miles from Washington. Much of the testing was done there. It was not until July 1943 that the Ordnance Department ranges at Aberdeen were finally ready. This delay in setting up adequate testing facilities was again indicative of the Army's lukewarm attitude toward proximity fuzes.

In addition to this assistance in field testing, Section E continued to help Signal Corps solve problems arising from production of the fuzes, particularly in improving laboratory acceptance test methods, especially microphonic tests, and in tube performance.

Signal Corps, in September 1942, further asked Section E to design and to construct a model of a Field Test Set that could be used in the field to test each of the fuze components, prior to assembly of the complete fuze and insertion into the projectile. Clarke completed a model by December and it was delivered to Signal Corps shortly thereafter. The test set was sturdy enough to withstand the severe treatment it would have under combat conditions. It also was simple and could be operated by personnel having no instructions in its use other than those provided with the set and who knew nothing of the principles of operation of the units being tested.

About 400,000 each of the radio and photoelectric fuzes were manufactured but none were used in combat as intended. During 1943 production was gradually tapered off. This was a devastating blow both to the development groups and to the manufacturers. Reasons were understandable, perhaps inevitable, but hard to take. The Army's 4½-inch rocket did not prove itself in, for a number of reasons, chief among which was its high dispersion and low velocity.

Section E found itself with an excellent fuze but no weapon upon which it might be used nor any admitted tactical need for the weapon had it been perfected. Signal Corps was little better off with 800,000 fuzes in warehouses. Seven disgruntled manufacturers swore they would never take another fuze contract. This all-out war effort was discouraging.

Eventually a few fuzes were used. Other applications were sought for the large stockpile of battery-powered rocket fuzes. Starting in March of 1943, tests were carried out at Fort Bragg, using radio-fuzed 4½-inch rockets against ground targets. Effect field studies showed that this combination was slightly better against shielded ground targets than the air burst of a 105-mm. howitzer shell. It appeared that the fuze would prove quite effective in barrage as a quick-blanketing weapon.

The weapon was demonstrated to officers of the First and Ninth Armies in ETO in January 1945. Orders were placed for about 75,000 fuzes. These were intended to be used in forcing the Rhine crossing but the crossing was made ahead of schedule by other means so the fuzes were not used. Small-scale use of them was made by the Fifth Army in MTO during March and April of 1945. Fuze performance was considered satisfactory but more extensive use was discouraged because of the high dispersion of the 4½-inch rocket.

The 4½-inch rocket equipped with the proximity fuze was also evaluated as an air-to-ground weapon. Against shielded targets the weapon was definitely superior to surface-burst rockets but inferior to equivalent loads of air-burst bombs. Only one air force, the First Tactical Air Force, used the 4½-inch rockets to any extent in the European theater. This air force tried about 50 of the radio fuzes during the last few weeks of the war. Their reports on the effectiveness of the weapon were very favorable.

CHAPTER XXII

BOMB FUZES

IN DECEMBER 1942, NDRC was reorganized and eighteen new Divisions were set up to replace the five Divisions of the old organization. For the most part, the new Divisions were made up from former Sections and combinations of Sections. As a result, Section E became Division 4. With the exception of transfer of work on controlled-trajectory bombs, there was no change in the technical program. Ellett was appointed Chief of the new Division on December 9. Members appointed a few days later were Briggs, Dryden, Coolidge, Diamond, and J. T. Tate. F. L. Hovde was appointed later to replace Tate, who resigned due to the pressure of other duties.

With the reorganization of NDRC in December, the Bureau of Standards organization was also changed. Briggs created a new Division, D-General, later Ordnance Development, and appointed Diamond as Chief. This Division was made of the various groups previously working under Ellett's direction, which now included about two hundred persons. From then on Ellett functioned only in an NDRC capacity.

Soon after the reorganization of NDRC, Ellett and Colonel Morton began to lay plans. In spite of the rocket fuze situation, they anticipated large requirements for many other applications, even though no Service requirements existed. As a result, the newly formed Division 4 met on December 24 and approved a much expanded program. An additional $750,000 was transferred to the Bureau of Standards for pilot-development and testing groups and for the purchase of model fuzes, fuze parts, projectiles, and test equipment.

The contract with Bell Telephone Laboratories was extended to cover development of a photoelectric fuze for use on the Army 500-pound semi-armor-piercing bomb in plane-to-plane bombing operations, power supply to be either a low-temperature battery or a wind-driven generator; of a radio fuze for the same application; and of a radio fuze for the jet-propelled armor-piercing bomb. The contract with the Friez Com-

pany was also extended. Friez had been co-operating with the National Bureau of Standards in furnishing mechanical and engineering services and model shop facilities.

There were many new contracts let. One was with the General Electric Company for the development at their Bridgeport plant of a radio fuze for use on a 100-pound bomb in plane-to-plane operations. Another, with the Philco Corporation for the development of a radio fuze for very large bombs. Reliable operation was stressed and limitations of size and weight practically nonexistent. Power supply for both fuzes was to be either a low-temperature battery or a wind-driven generator. A contract was placed with the General Electric Company for the development at Schenectady of an ultra-high-frequency triode. Westinghouse was given a contract for the development at their Mansfield plant of a condenser-powered photoelectric rocket fuze. Lastly, a contract was placed with Emerson for the development of a pulsed-type radio fuze. This was intended to operate on the radar principle measuring the time between the emission of a pulse and the return of the reflected signal.

Other contracts were extended, with the Sylvania Company covering the development of heater-type tubes, and with Raytheon covering development of an ultra-high-frequency diode. The contract with Westinghouse was also extended to cover development of a radio fuze for operation against ground in 100-pound Chemical Warfare bombs, plus the development of a wind-driven generator for all bomb fuzes.

On March 20, Division 4 met again to review its program. As a result, work on radio fuzes for very large bombs, for the jet-accelerated bomb, and for high-altitude aircraft rockets and work on the condenser-powered photoelectric fuze were terminated. Principal effort was to be concentrated on radio bomb fuzes.

This expanded program paid dividends. It greatly increased the facilities at the disposal of Division 4. Engineers at the various commercial contractors had had an opportunity to work closely with fuze-development engineers at the National Bureau of Standards and acquired much basic fuze technology. Much was accomplished in compromising desired military characteristics with those which could be obtained practically. Thus, in June when the Services asked Division 4 to undertake an urgent bomb fuze development and procurement

program the Division had the know-how and facilities to undertake it immediately.

Work on photoelectric fuzes was terminated in August 1943. Service interest was low and the men working on the project were needed in connection with work on radio fuzes.

During this same period Clarke was crusading for a wind-driven generator power supply. The type of dry battery used in the rocket fuze was far from satisfactory. It deteriorated rapidly in storage. Under ideal storage conditions its life was about a year; under conditions of high temperature and humidity its life was not more than one or two months. Dry batteries became inoperative at sub-zero temperatures. Work on a reserve-type battery, having good low-temperature operating characteristics, was not showing much promise.

Clarke recognized, perhaps more clearly than anyone else, that a wind-driven generator would have none of these disadvantages and would have the added advantage of increased safety, since the rotating system of the generator could be used to arm the fuze both mechanically and electrically. Moreover, there would be no electrical power to operate the fuze unless the vane were turning. Firmly convinced that a wind-driven generator could be built in the space allotted the battery, Clarke devoted himself to an intensive study of the problem. He worked out a complete design for a generator-powered bomb fuze.

The generator was small. It had rugged design, stable operation, and simple construction. An electrical regulation circuit was developed which maintained constant generator power output even though the rotational speed of the generator varied over a range of three to one. The production design of the generator, developed by George Morris of the Zenith Radio Corporation, was even smaller and proved even more satisfactory.

The development of the generator was a milestone in proximity fuze design, but it was not an unmixed blessing. Normal commercial machinery runs at speeds not greater than 3600 revolutions per minute; high-speed tools run at about 12,000 rpm. In these small generators, design considerations required speeds up to 50,000 rpm. At such speeds mechanical stresses and the resulting vibrations are great. Much effort was devoted to bearing design, to balancing the rotating mechanism, and to improving the rotor structure so that it would not fly apart at

such high speeds. It should be noted that all of these problems were eventually solved.

During April and May of 1943 things were coming to a head. The design of a workable bomb fuze was complete. In February the National Bureau of Standards had completed drawings of this fuze and drawn up a bill of material. During March a number of models of this fuze were made and tested extensively in the laboratory. On April 7 five complete fuzes were dropped at Aberdeen. All five functioned on arming rather than against the ground, and most of April was spent in trying to find reasons for this malfunction and in modifying the design to correct it. Some fifty fuzes were dropped in May, with proper performance running about 50 per cent; but by the end of June, 80 per cent performance had been realized in some tests. Little attention was paid to large-scale production problems. Main interest lay in the proving in of the basic design.

Satisfactory components for use in the fuzes were in sight. The vacuum tubes used in the battery-powered fuzes operated satisfactorily in the generator-powered fuzes. These tubes were in large-scale production for Signal Corps at Raytheon, Sylvania and General Electric.

E. A. Hardy at General Electric's East Lynn plant was just getting started on development of a rectifier cell which proved satisfactory and eventually was used almost exclusively.

Reduction-gear trains were being produced in small quantities by the Belmet Products Company, and Globe-Union was asked to begin designing an improved gear train. Solar and Cornell-Dubilier were supplying special filter condensers.

The small rotors for the generator were supplied by Arnold Engineering Company. Zenith had completed the design and was tooling for a generator stator designed for mass production. Knapp-Monarch was well along on the design and tooling of a generator. The bearing problem had been investigated and sources of supply for oilite bearings had been set up. Several thousand generator coils and lamination assemblies were on hand.

Anticipating a large experimental production program and recognizing the serious delays often encountered in procurement of parts, Ellett had asked the National Bureau of Standards, in February, to set up a stock of electrical parts sufficient to build 5000 fuzes. Most of these were

on hand. All of the small resistors and condensers needed were in large-scale production, and tooling for large-scale production of the other parts was under way.

The success of the generator-type fuze in proving-ground tests made it clear that the time was ripe for initiating large-scale experimental production. Clarke had, in March and April, made an extensive survey of facilities with a view to selecting several for these operations. Active negotiations began the first part of May and after they were well along a meeting of Division 4 was called for the tenth of May, 1943. The contemplated experimental production program was discussed. The general purpose of it was the elimination of numerous minor accidental causes of malfunction. A considerable number of fuzes was necessary for an adequate testing program. Techniques of assembly, assembly-line and acceptance-test methods and equipment had to be proved-in. Such production provided the only sound basis for specifications for large-scale procurement.

Contractors were chosen from those having engineering groups with experience in fuze development and having potential production capacity.

A group at Philco, headed by Maurice Swift, had engineered and put into production the battery-powered radio fuze for the 4½-inch rocket and had already worked some months on the development of a battery-powered fuze for use on large bombs. On June 1, Philco started work on a lot of twelve fuzes, following closely the Bureau design and using their parts. The first model was delivered on July 1 and six more on July 6.

General Electric had already begun work on the generator-powered bomb fuze. Initial progress was rapid; by March their drawings were almost complete. On March 6, the contractor was authorized to procure parts and materials and to make or procure tools for production of 10,000 fuzes in small lots of 100.

Harmon's group at Westinghouse had been working closely with the Bureau on radio fuze problems since 1941. They were well along on the design of a transverse-antenna generator-powered radio bomb fuze.

Zenith was not approached on fuze development until March of 1943. G. E. Gustafson, their Vice-President in charge of Engineering, took a keen and personal interest in the project. As a result, by May

Zenith had completed a model of a generator-powered rocket fuze. This model incorporated much ingenious engineering.

Dorman Israel's group at Emerson had proved very flexible in connection with additional problems relating to the radio fuze which it had been working on under OSRD development contract since January. On June 22, two rocket fuzes constructed by Emerson were laboratory-tested at the Bureau of Standards. At that time 25 more were under construction. Circuits for the bomb and rocket fuzes were similar and many of the mechanical parts were interchangeable. Although Emerson engineers had had no experience with bomb fuzes before July, they were familiar with much of the basic technology.

Friez engineers had worked closely with the Bureau on problems of the radio fuze since the very beginning. Knapp-Monarch was chosen because of its broad experience in large-scale production of low-cost electrical equipment. It had been working on the development of a suitable generator since April 1943.

The need for much more extensive machine-shop and assembly facilities was met by a contract with the Zell Corporation in Baltimore, and a small line had begun production.

Early in February, when drawings for the bomb fuze were first completed, it was decided that metal and mechanical parts for 1000 fuzes be procured. Construction of many of the dies and molds for producing these parts and fabrication of the parts was undertaken by Zell. Thus, Zell had completed considerable tooling and had delivered sizable quantities of many of the parts. It was in a position to produce parts for its own use and for use by other Division 4 contractors in the experimental production program.

In March Zell began to construct oscillators and amplifiers and other subassemblies for use by the Bureau. In July Zell began complete assembly of bomb fuzes. Their production line was equipped with testing and all other facilities needed to make it a self-contained plant capable of performing all operations necessary to fuze manufacture. Construction of the first lot of 50 fuzes was started on June 28.

A production-engineering and quality-control group had been set up in March at the National Bureau of Standards. It handled preparation of specifications, quality control of components, inspection of parts, routine laboratory testing, design of production test equipment, prepa-

ration of units for field test, and analysis of laboratory and field data, and assisted manufacturers in production problems. By July the group was well organized and functioning.

It was recognized at the outset that successful completion of the development of the bomb fuze would require the closest of co-operation between Service branches and Division 4. A general plan was formulated by which Emerson and Philco were to be the two principal producers. While initial progress at GE had been rapid, after March they had bogged down. Although development was continued at this facility, it ceased to be considered as one of the first producers. Management at Friez was no longer interested in fuze development and production. Zenith and Westinghouse were not yet far enough along to be considered good prospects.

A design suitable for large-scale production was worked out as rapidly as possible and each contractor was to start, as soon as the production design was complete, sufficient tooling to support production of 5000 to 10,000 fuzes per month. In the meantime, Philco and Emerson were to begin production with parts supplied by Division 4.

Contracts were placed by Signal Corps with each of the contractors for procurement of large quantities as soon as requirements were set up by the using arms. As soon as satisfactory fuzes were being produced at a rate of 100 to 200 a day, the production facilities were to be transferred to Signal Corps.

The Ordnance Department followed the program closely, assisting particularly with the design of the arming and safety features. It made recommendations regarding military characteristics and provided facilities for necessary special testing.

To arrive most quickly at a single production design or, at most, two production designs for the fuze, a Standardization Committee was set up in early July 1943. This Committee consisted of six members as follows: Alexander Ellett, Chairman; John S. Rinehart, Secretary; A. S. Clarke, Technical Aide, Division 4; Harry Diamond, Chief of Bureau group; R. B. Battle, Philco Corporation; and Dorman Israel, Emerson Radio and Phonograph Corporation. Each member was free to bring to any meeting such engineers as he wished and, in fact, several engineers from each development organization usually attended. The first meeting was held at the National Bureau of Standards on July 12,

1943. Weekly meetings were held until October for the purpose of discussing design features.

The opportunity afforded for exchanging of technical information, settling differences of opinions, and for those in charge to follow closely the progress of the several development groups did much to speed up the program.

The activities of these several groups who participated in the experimental production resulted in large-scale production of the longitudinal bomb fuze by June 1944.

As Division 4's Central Development Laboratory, the National Bureau of Standards played a major role. During the period from July 1943 through June 1944 practically all work there was directed toward bomb fuze problems. The contributions made by the Bureau group were vital. They proved-in a basic design for the fuze and contributed heavily in the production design. They rendered valuable assistance to component suppliers in the development of special components such as selenium cells, filter condensers, gear trains, generators, and bearings, and conducted extensive laboratory tests for manufacturers on fuze models, subassemblies, and components. All quality control and field testing of completed fuzes was handled by the Bureau, and early in 1944 Signal Corps transferred $150,000 for setting up a Quality Control Group. Literally thousands of parts for fuzes were in their stockroom. They assisted in the preparation of an Ordnance Technical Manual on the fuze and co-operated in setting up a training program for Ordnance personnel.

During this same period, the Zell Corporation assembled and delivered some 11,300 bomb fuzes. Their facilities played an important role in the fuze program in that their availability made it possible for the developmental engineers to prove-in design modifications without interfering with production at Emerson and at Philco.

The majority of the mechanical parts and a great many of the molded parts used by Emerson and Philco were produced at Zell.

By November 1943, it was apparent that facilities at Zell were inadequate. An extensive survey of facilities in the Washington area resulted in Division 4's placing a contract with Bowen and Company in Bethesda, Maryland. By mid-February 1944, Bowen had assembled the necessary facilities for a complete fuze plant and had employed Clarke as Chief

Engineer. The first completed unit was shipped from Bowen March 21. With the exception of electrical components, such as resistors, condensers, vacuum tubes, alnico rotors, and wound stators, every mechanical part used in the device was produced by them.

By May, the Company was producing fuzes at a rate of about 300 to 350 per week. Production continued at about this rate through November 1944, totaling about 10,000 fuzes. Production of these fuzes served principally to prove-in design modifications without disturbing large-scale production. Many of these changes were later incorporated in the Emerson and Philco designs. In addition to producing fuze parts for its own use, Bowen supplied large quantities to Emerson. Initial quantities of a great many of the bottleneck mechanical parts for production were produced there.

Emerson engineers were shown models of the National Bureau of Standards bomb fuze at the July 7, 1943, meeting of the Standardization Committee. Although they had undoubtedly discussed the fuze informally with members of the Bureau staff previously, this was their first look at it from the point of view of an ultimate producer. The first 20 fuzes were delivered on September 23. By the first of January, Emerson was making about 20 fuzes per day. The rate was increased gradually to about 50 fuzes by the end of March. Complete tooling for the radio fuze and a supply of parts adequate to support production were initially expected by February. However, a number of bottlenecks developed, particularly with respect to molded parts, and in spite of extensive expediting by the Army Air Forces, the Ordnance Department, the Signal Corps, and OSRD, the tooling program moved slowly. Emerson was not in a position to begin large-scale production until the first of May.

During the period from May 5 to June 12, 1944, Emerson produced about 7000 fuzes, of which about 4500 were considered acceptable. Of these, 2000 were delivered to the Services. By the first week in June, Emerson was producing 500 fuzes per day with rejects at the final test position approximately 30 per cent and performance of the fuzes in the field 80 to 85 per cent. The production line was well stabilized and on June 12 the Emerson facilities were transferred to Signal Corps.

Emerson was slow in getting into production. A principal difficulty was a lack of parts arising from unwise choices in selecting suppliers

and from lack of adequate initial priority support from the Services. In addition, there was a lack of adequate inspection and quality control of incoming parts. It was necessary to change some of the component designs which, because of the urgency of the program, were not fully tested before placing orders. In March, Hinman of the Bureau was detailed to Emerson for a period of about three months with responsibility to act for Division 4 on engineering and production problems. Through close co-operation, difficulties were relieved and acceptable production obtained.

At Philco, initial progress was rapid. Five fuzes were delivered on August 6, 1943; five on August 10; fifteen on August 30; and seventeen on September 6. By then manufacturing drawings for the production model were substantially complete and Philco was authorized to tool immediately for production of 10,000 fuzes per month. From then on through January 1944, progress was slow. This appeared to be due to the failure of their production and engineering staff to appreciate the problems peculiar to the manufacture of proximity fuzes. There was some reluctance to begin production in order to prove-in production techniques, to test specifications, and to determine any necessary design changes.

Since the urgent requirements for bomb fuzes demanded the most expeditious action, a conference was held December 11, 1943, between Signal Corps, Division 4, Ordnance Department, and Philco representatives, to discuss the Philco program. It was made clear that the primary concern was to obtain fuzes at the earliest possible date. OSRD withdrew from Philco as far as experimental production was concerned, turning it over to Signal Corps. While work at the Philco Corporation continued under Signal Corps direction, it was not until late fall that production got under way there.

At the May 10 meeting of Division 4, Colonel Morton stated that, although no large requirements existed for rocket and bomb proximity fuzes, only by anticipating the requirements of the using arms, and proceeding with development and production on the assumption that requirements would be established, would it be possible to have fuzes ready. He urged Division 4 to push vigorously experimental production of all types.

A memorandum from the Air Ordnance Officer to the Chief of Ord-

nance, dated July 3, 1943, clarified the situation considerably and formed the basis for Division 4's all-out effort on the bomb fuze. It specified the types of bomb on which the fuzes were to be used and initially requested 150,000 proximity fuzes. In addition, Navy requirements were set up in a memorandum, dated July 5, 1943, from the Chief of the Bureau of Ordnance to the Chief of Ordnance estimating an initial 50,000 fuzes. The Ordnance Department formally approved in July 1943 this limited procurement.

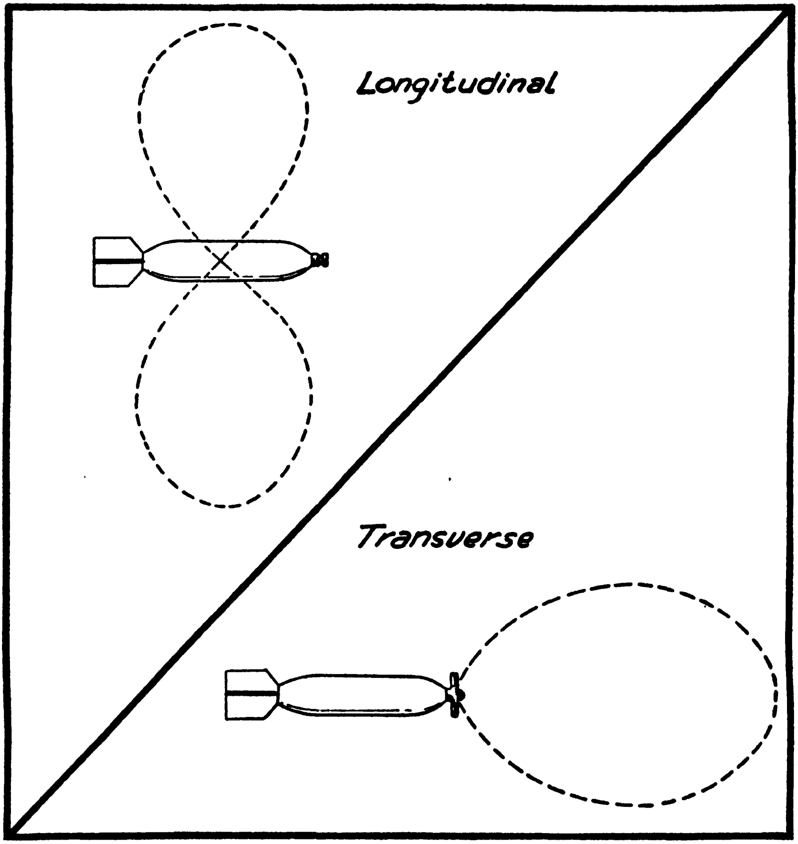
Representatives of the Ordnance Department participated actively in the bomb fuze program from its initiation, the officers most active being Colonel H. S. Morton and his associates. Much of the success of the production program was due to the extremely fine relationships which existed at all times between the Ordnance Department personnel and Division 4 personnel, and in particular, to Colonel Morton's and Ellett's mutual appreciation of the problem involved.

Specifically, the Ordnance Department clarified and interpreted Service requirements. Its contributions in connection with the arming and safety problems were invaluable. Ordnance also arranged for field testing of the fuzes at Aberdeen and Eglin Field, expedited materials and equipment, arranged for loading of explosive parts at Picatinny, and designed packaging for the fuze.

Large-scale procurement was initiated in November 1943. The Ordnance Committee approved, on November 7, limited procurement of 1,075,000 radio bomb fuzes. As procurement agency for the fuzes, Signal Corps worked closely with Division 4's Central Development Laboratory at the Bureau regarding technical details. They kept in touch with the Ordnance Department regarding military characteristics and Service requirements. Procurement was handled by the Monmouth Signal Corps Procurement District and technical phases of the procurement by Camp Evans Signal Laboratory. Lieutenant M. P. Rome, MSCPD, was Contracting Officer on all contracts; Captain G. K. Green, CESL, handled technical problems.

Real impetus was given to the program in April 1944. In connection with preparations for the invasion of Europe, Air Force Commanders in ETO became keenly interested and requested immediate delivery of 10,000 bomb fuzes. At a meeting on April 19 between Air Ordnance, Signal Corps, Ordnance Department, and Division 4, representatives

agreed to direct all effort toward fulfilling the request. Division 4 was to furnish a minimum of 4000. Others were to come from Signal Corps production. By June 13 Division 4 had delivered 4829 fuzes.



National Bureau of Standards

Envelopes of the sensitivity pattern for longitudinal and transverse fuzes

The Ordnance Committee authorized procurement of radio bomb fuzes of either the longitudinal type or of the transverse-antenna type. The directional properties of the antenna on the longitudinal fuze give good sensitivity against aircraft in the direction where maximum

damage is expected to be caused by the rocket or shell. Against ground targets, however, the sensitivity of the longitudinal fuze is lower than that of the transverse fuze. This difference is illustrated in the drawing on page 208. The transverse fuze has the further advantage that its circuit constants are much more independent of the size and shape of the missile to which it is attached. It can therefore be more nearly an all-purpose fuze. The same fuze can fit several different sizes of missile.

Effort was centered, until June 1944, on getting the longitudinal type into production. Attention was then turned to the transverse-antenna fuze. Previous to this time, development of transverse fuzes was proceeding at Zenith, Westinghouse, and at the National Bureau of Standards. The type under development at Zenith appeared most promising. This design followed NBS recommendations and made maximum use of parts from the longitudinal fuze. It had a propeller-driven generator and incorporated the power supply used in the production model of the longitudinal radio bomb fuze.

Zenith had begun work on the transverse bomb fuze in September 1943. They had acquired basic fuze technology in the development of a generator-powered rocket fuze and, in particular, in the development of small generators. A production design was completed by June 1944 and several small lots were built which gave a continually increasing standard of performance in the field. Quality was consistently high. By July, Zenith was averaging 50 fuzes a week from parts made from temporary tools. Production tooling was essentially complete by October and production increased to about 100 per day, increasing in December to 200. By January they had delivered approximately 10,000 fuzes, and Signal Corps took the facility over on January 1, 1945; large-scale production began and continued until the end of the war.

Progress at Westinghouse was slow. Because of its great familiarity with problems peculiar to electronic fuzes, the Baltimore group undertook the development of a transverse-antenna battery-powered bomb fuze. Several fuzes had been built in the laboratory and tested successfully in the field, and Westinghouse was to initiate experimental production as soon as a preliminary design had been worked out. It was anticipated that this production would be at their Mansfield plant, with

the Baltimore group carrying the burden of the development and providing all engineering supervision. Production drawings were completed and production was scheduled to start.

However, tests of a few complete units demonstrated that the basic design was not sound; production drawings for a modified design were completed. Again Westinghouse was authorized to proceed with tooling. Small lots of laboratory models gave, however, satisfactory performance in the field. By March 1945, tooling was not yet complete and in view of the impending demobilization of OSRD, the Ordnance Department agreed to take over the facility. This transfer was effected April 1, 1945.

Extensive field tests of the Zenith fuzes showed that they functioned properly on all bombs from 100-lb. to 4000-lb. sizes, at heights somewhat greater than were attainable with the longitudinal fuze. Burst heights were relatively independent of release altitude.

Chief among minor modifications was a device which provided additional delay in arming. It could be adjusted in the field to permit the fuzes to be dropped safely through deep formations of bombers. It consisted of a small auxiliary vane and gear train externally clipped to the fuze, which released the fuze vane after a preselected amount of air travel. The device was designed by the National Bureau of Standards and was engineered for production by Globe-Union, Inc. All bomb fuzes, both longitudinal and transverse, were subsequently equipped with this device.

Airplanes, trained pilots, and large proof areas were required for the testing of bomb fuzes. For these reasons, Division 4 made no attempt to set up its own facilities for the testing, but used Army and Navy test facilities. In general, the branch of the Services most interested in a particular fuze development provided facilities for testing it. Early testing was done at Dahlgren, and when the development of a Chemical Warfare bomb fuze was well along, Colonel Morton arranged for its testing at Edgewood, Maryland, and Dugway, Utah.

From a rather unpretentious beginning in the spring of 1943, the testing of bomb fuzes grew by the summer and fall of 1944 into a major operation. All told, some 7000 tons of bombs were dropped. A special range was set up and two or three bombers were made available at Aberdeen. The National Bureau of Standards Test Group maintained

a staff of ten to fifteen persons to supervise the installation of fuzes in bombs and to make observations on all rounds tested. On clear days as many as 100 to 150 bombs were dropped. However, a clear day was a rarity. From the standpoint of flying weather, it would be difficult to choose a place worse than Aberdeen. Testing lagged months behind production, and additional planes, so badly needed, could not be obtained.

In February 1944, at the request of the Air Forces, a few longitudinal radio fuzes were tested at Eglin Field and the quality of performance aroused a greater interest in them. In March, the Army Air Forces were invited to participate in the final development testing prior to freezing the design, and during April and May, fuzes were dropped against an effect field to assess the relative potency of equal airplane loads of clusters of 20-lb. bombs, general-purpose bombs, and radio-fuzed fragmentation bombs when used against personnel and light materiel. Results of the tests were contrary to general expectations and theoretical predictions, and while the Air Forces admitted a "slight superiority" they stated the advantage was nullified by the high percentage of early bursts inherent in the radio fuzes.

Firmly convinced that the tests at Eglin Field had been badly handled, Ellett defended the usefulness of air-burst bombs. He felt that the targets at Eglin did not simulate actual tactical conditions and that the analysis of the results was in error. The bombs were released in salvo or close train, resulting in extensive overhitting. Many of the "casualties" were due to two or three bombs as there was much overlapping of the fragment patterns. Every piece of target cloth pierced was counted a casualty, with no means of determining whether the hole was due to a lethal fragment or to a high-flying clod thrown out of the crater of an impact-fuzed bomb. Under the circumstances, further tests seemed to be desirable, and they were repeated at Eglin in the fall of 1944 under more carefully controlled conditions. This time the great advantage of air burst was demonstrated. Unfortunately the early adverse report, which received wide distribution to commanders in the field, delayed operational use of the fuze for many months. In September 1944 and a few months before the tests were repeated at Eglin Field, the British initiated an extensive effect-field test using Division 4 fuzes. The results of these tests demonstrated a very decided superiority for air-burst

bombs against shielded targets. A. V. Astin of the NBS staff participated in these tests as a representative of Division 4.

The proximity fuzes developed by Division 4 saw very limited operational use. Extensive employment of these weapons was handicapped by a combination of security restrictions and varying tactical requirements. Wherever the fuzes were used, however, the reaction was very favorable, and enthusiastic reports concerning their effectiveness came in from the various theaters of operations. General release of proximity fuzes for use over enemy territory was made by the combined Chiefs of Staff on October 25, 1944.

The principal objection to the general release came from the Air Force Headquarters. It was generally believed that their use over enemy territory would lead to recovery and copying of the fuzes by the enemy. So effective is the proximity fuze as an antiaircraft weapon, that had the enemy been able to use them against us, the Allied air offensive would have been considerably handicapped. Fuze experts estimated that it would take the enemy at least eighteen months to duplicate fuzes, and with opinions that the war in Europe would end in 1945, as well as with the great inherent advantage in the weapon itself, general release followed.

Army Air Force Headquarters was still not prepared to endorse the immediate use of proximity fuzes. On the contrary, it sent cables to the theater air commanders, almost simultaneously with the general release, stating that there was no appreciable advantage in the air-burst bombs and that the fuzes available were not suitable for operational use. The conclusions of the cable were based on the early tests carried out at Eglin Field as discussed above. It was not until the latter part of April 1945 that the AAF notified the theaters of the results of the additional and successful tests carried out in the fall of 1944, and removed all doubts cast by the October 1944 cable.

The introduction of any new Ordnance item requires advance training. This was particularly true of devices as novel as proximity fuzes. A training course in the operation and use of bomb and rocket fuzes was set up at the Bureau of Standards and Aberdeen Proving Ground starting January 16, 1945, and ran for approximately three weeks. Brigadier General Coupland, the Air Ordnance Officer, arranged for teams of officers and enlisted men from each of the numbered air

forces to attend this school. Officers from the Navy also attended. The course consisted of a week of intensive study of the theory and practice of fuze design at the Bureau, followed by two weeks of demonstrations and training in the use of the fuzes at Aberdeen, after which the officers and men returned to their respective stations.

In late 1944, E. L. Bowles, Expert Consultant to the Secretary of War, toured the Pacific area, explaining the military possibilities of various new technical developments. He included discussion on proximity fuzes for bombs and was able to clarify the misunderstandings about the fuzes which had arisen following transmission of the first Eglin Field data. Following Bowles's visit the Fifth and Seventh Air Forces requested that trial lots of fuzes be shipped to the theaters by air, together with experts to advise on their use and handling.

On January 10, 1945, F. S. Atchison of the Bureau and Lieutenant E. J. B. Stearns of the Ordnance Department arrived in Saipan in response to the request. A successful demonstration was held at Saipan January 22, 1945, for representatives of the Seventh Air Force and its various bomber groups. Following it, plans were laid for the use of proximity fuzes in the bombardment of Iwo Jima. This was the first operational use in the war of bombs fitted with proximity fuzes.

Three missions were carried out over Iwo Jima February 10, 11, and 17. The first two were against antiaircraft gun positions and the latter against the beach defenses. Antiaircraft fire diminished following the operations and remained at greatly lowered intensity up to the actual invasion of the island on February 22.

First use of proximity fuzes by the Fifth Air Force was on March 23, 1945, against the town of Legaspi, Luzon, P.I. A total of 88 fuzes were used on 1000- and 2000-lb. bombs, but since 30 per cent of the total tonnage dropped on the mission was proximity-fuzed, and since photographic coverage was incomplete, it was impossible to evaluate the effectiveness of the new weapon.

Reaction to the initial use of these fuzes was very favorable and orders were placed for large quantities. These orders were shipped by boat and did not arrive in the theater until just before the end of the war. In the interim, small quantities of fuzes which had been shipped by air were used with success by the Army Air Forces on Palau, Marcus Island, and on the Japanese homeland. Favorite targets were gun em-

placements and airfields. Only a few hundred fuzes were available for these interim missions and they were used largely for training purposes in anticipation of an all-out use in support of the expected invasion of Japan.

Following the release of the fuzes, A. V. Astin, who was in ETO in connection with the British tests of Division 4 fuzes, visited Ninth Air Force Headquarters in France to discuss possible requirements and also to disclose results of the British evaluation at Ashley Walk, demonstrating the superiority of air-burst fragmentation bombs. Following this visit, the Ninth Bomber Command forwarded a request for the immediate delivery of 10,000 radio bomb fuzes. The order arrived at U.S. Strategic Air Force (USSTAF) Headquarters shortly after the cable from Army Air Force Headquarters which stated that the fuzes were not suited for operational use, and as a result the order was canceled.

On December 14, 1944, Ellett flew to Europe to make a survey of British and American requirements. He found considerable interest in British circles and indication that sizable requirements would be placed for American fuzes. He visited Air Force officials at USSTAF, Eighth Air Force, Ninth Air Force, Ninth Bomber Command, and Ninth Fighter Command, and found that, although the AAF Headquarters cable had forestalled immediate operational use of the fuzes in the theaters, there was sufficient interest in the air-burst bombs to warrant preoperational trials of them. Also, the Air Forces were persuaded to send some of their best ordnance officers to the school on proximity fuzes in the States.

Early in March, limited quantities of fuzes arrived in ETO and the Ninth Bomber Command planned a number of preoperational trials. The fuzes were used on 260-lb. fragmentation bombs, and dropped in-train from a single bomber and from six bombers in tight formation. Strike photos were taken to observe fuze performance. Operation was so satisfactory, with the target area so densely covered with fragments, that it was concluded that adequate effect could be obtained by using only three bombers in a loose formation. Operational use was started on March 15 using the air-burst bombs to neutralize antiaircraft gun positions at Pirmasens and Neunkirchen, Germany. Results were excellent and the Ninth Bomber Command continued to use the available

fuzes, about 1300 in all. Following the preoperational trials, orders were placed for large quantities of additional fuzes, but these did not arrive before V-E Day.

The main interest of the Eighth Air Force in proximity fuzes was in their application to the air bursting of large fire bombs. A series of tests were held in January 1945 using the fuze in the 165-gallon Napalm-filled bomb. Though there was an increased spread of fire over contact fuzing for high-altitude bombing, the bombs could be carried only externally on fighter planes and were not suitable to the operational plans of the Eighth Air Force. Work was then directed to the development of a suitable container for Napalm to be carried in the bomb bays of heavy bombers. This investigation was still in progress on V-E Day.

In MTO, preliminary instruction and briefing on the use of proximity fuzes for bombs and rockets was carried out by Captain Walter G. Finch of the Ordnance Department. Captain Finch was head of proximity shell fuze teams in MTO but he had also familiarized himself with bomb fuzes before leaving the States. Again the AAF cable of October 1944 caused considerable delay in the initial use of fuzes. After small quantities of fuzes were ordered and tested, they were used operationally April 1, 1945, by the Fifteenth Air Force. During April, this same Air Force used approximately 1500 fuzes, against German flak batteries located near Grisobra, Italy.

The Twelfth Air Force used approximately 100 fuzes in fragmentation bombs, general-purpose bombs, and the 165-gallon fuel tank incendiary bomb. Following these trial uses of the fuzes, MTO placed orders for large quantities. The end of the European war in May 1945 precluded further use.

Use of proximity fuzes in the CBI theater was confined to operational trials with sample lots. All trials were successful and requests for large orders followed initial use. About 600 fuzes were expended by the Tenth Air Force, the Twentieth Air Force, the 301st Fighter Wing, and the Fourteenth Air Force. No malfunctions were observed.

The carrier-borne aircraft of the Navy started use of proximity-fuzed bombs in May 1945 and employed them in appreciable quantities. It is roughly estimated that the Navy's use of the fuzes immediately preceding the capitulation of Japan exceeded the combined total used by the Army Air Forces during and prior to this period.

The Navy used these fuzes against antiaircraft gun positions, revetted aircraft, light buildings, and against personnel in the open. For example, the aircraft carrier U.S.S. *Randolph* dropped, from July 1 through August 15, 1945, a total of 2240 bombs of all sizes over Japanese targets. Of this number, approximately 800, or 35 per cent, of the bombs were radio-fuzed. The U.S.S. *Bennington,* U.S.S. *Independence,* U.S.S. *San Jacinto,* and the U.S.S. *Shangri-La* radio-fuzed between 30 and 37 per cent of their bombs. Reports of action for the Okinawa operations, for the attack on Ninami Shima and Kita Shima, and the strike on Wake Island revealed extensive damage.

CHAPTER XXIII

MORTAR FUZE DEVELOPMENT

MORTAR fuzes were developed too late to see operational use. However, the story of their development is so representative of what could have been accomplished had the Army shown real enthusiasm for the other fuze programs that it seems worth relating. This program eventually received wholehearted support from the Services.

By the summer of 1943, Ellett felt that fuze designs had progressed to a point where a much smaller fuze appeared possible and he began talking with Colonel Morton and others in the Ordnance Department about development of fuzes for trench mortar projectiles and small fragmentation bombs. In November, the Army officially requested Division 4 to undertake their development.

Every fuze engineer was being overworked on the bomb job. No experienced ones were available to start on a new fuze. Fortunately there was a group at the University of Florida which had been working on an electric time fuze, and they were asked to begin investigating, in a preliminary way, possibilities of making smaller fuzes with particular emphasis on the mortar application.

The design of this fuze required the solution of two major problems: ruggedness and size. Whereas the early rocket fuzes were designed to withstand a shock 1000 times the force of gravity and the bomb fuzes had to stand up under rough handling, a mortar fuze must withstand a firing shock of 10,000 times the force of gravity. In addition, mortar projectiles are so small that fuzes of the size of those used on a bomb would spoil the flight of the projectile and make the round useless.

The group at Florida was small and inexperienced in fuze design, so progress was slow. Their first 30 models when tested in the field gave about 30 per cent performance. But most important, Sam Goethe, Alfred Khourie, and Alfred Tedder, three competent young engineers, were gaining experience in fuze design.

It was not until November 1944 that definite although small requirements for such a fuze were established by Ordnance Committee action and it was not until March 1945 that these requirements were increased sufficiently to be of interest commercially.

It was apparent that the ultimate reduction in size of fuzes would be limited by the size of the electrical components. Even though extremely small resistors and condensers were being used these still took too much room. It occurred to Ellett that, by using techniques known to ceramic electrical-component manufacturers, it might be possible to manufacture a complete circuit, except for vacuum tubes, as a single ceramic part, thereby obtaining a material saving in space and eliminating the necessity of making numerous soldered connections in assembly.

Ellett proposed the idea to C. O. Wanvig, President of Globe-Union. After consultation with Milton Ehlers, his chief ceramic engineer, Wanvig told Ellett that he thought production of such integral ceramic circuit networks practical and indicated a willingness to undertake a preliminary investigation. Globe-Union made up a number of complete amplifiers and oscillators using this process and were enthusiastic over its potential possibilities. They were also anxious to try their hand at designing a complete fuze. This job was assigned to H. L. White, working closely with Bureau of Standards engineers. White was already familiar with fuze problems, having participated extensively in the design of safety and arming mechanisms for rocket fuzes.

Early design work had followed two general patterns. The University of Florida group had a fuze with a loop antenna. A group at the Bureau had worked on a design which utilized the body of the projectile as a dipole antenna. The Florida design had certain points of superiority. The generator was at the rear of the fuze, making it possible to use a directly driven instead of a spring-loaded arming device, and the loop-type antenna had about the same sensitivity for objects alongside the fuze as for those ahead. On the other hand, the design of the dipole fuze utilized design principles already well proved-in. A first model of a fuze designed for use on the 81-mm. trench mortar projectile was completed by Globe-Union.

At this point the development was practically stopped because of changing Service interests. The Navy was anxious to get a fuze for its

5-inch rocket (HVAR). As a result development was proceeding along two lines, namely, (1) as a temporary expedient, modification of the bomb fuze for use on the rocket, and (2) as a longer-range program, development of a generator-powered fuze having much smaller physical size than the bomb fuzes. In view of the existence of definite requirements for this rocket fuze and the nonexistence of definite requirements for the mortar fuze, Ellett, at the request of the Ordnance Department, instructed Globe-Union September 18, 1944, to convert its fuze to a rocket fuze.

In addition, the Ordnance Department was disappointed in the outcome of the 4½-inch rocket and fuze program and did not want to get caught holding the bag with another rocket fuze. Accordingly, they were extremely anxious to develop, if possible, a so-called universal fuze that could be used interchangeably on all types of Army and Navy rockets.

Ellett discouraged this development on the grounds that the widely varying characteristics of the various rockets made it impractical to develop "the universal fuze." Nevertheless, Ordnance Department thinking reached Globe-Union with the result that Globe-Union progress was delayed in attempting to modify their designs to incorporate such universal characteristics.

The Army remained reluctant to back up an all-out program even though, in December, there was an increased interest in mortar fuzes. Ellett was firmly convinced that only by such an all-out program would the fuze get into use in this war.

Ellett went to Bush, urging that one of the three following courses be taken: (1) that OSRD drop the development, (2) that OSRD prosecute the development vigorously to the extent of producing some 50,000 to 100,000 fuzes on crash basis, or (3) that strong Army support be obtained for the program through immediate placement of large orders.

Bush decided to follow the latter course and on March 17, 1945, wrote to General Borden: —

> The program is one of very large magnitude. The time-scale that it is necessary to reach in order to be fully in time is highly compressed. It can be carried through successfully only if definite decision is made at the top

to accept the risks involved, and if this decision is communicated to every officer involved in order to further the subject vigorously at every point and clear obstacles out of the way.

I would not be completely frank with you if I did not express the serious doubt in my mind whether the Army can carry this program through to a successful conclusion. I know what was involved in putting antiaircraft fuzes into the Pacific and howitzer fuzes into action on the Western Front. I know also the obstacles that are in the way, for I met them fully in the case of such devices as the spherical hand-grenade. Nevertheless, if the Army wishes to do this job on an accelerated schedule, and to put the full weight of the General Staff behind the project at every point, this office will do its full part to the best of its ability.

The War Department took action. General L. H. Campbell, Chief of Ordnance, called a meeting of NDRC and Service representatives March 31, 1945, to discuss ways and means of getting trench mortar fuzes into production as rapidly as possible. The job became the Number One job of the Ordnance Department. The meeting was heavy with brass, with everyone enthusiastic and eager.

The goal set was 400,000 fuzes per month by January 1946. This meant completion of development and freezing of production designs by July.

At this time Globe-Union was making fuzes at the rate of about 10 a day. The latest lot of 25 fuzes had given 65 per cent satisfactory performance, which was typical of early production from temporary tools. It was not clear whether the Globe-Union ceramic construction was capable of large-scale production. As a safeguard, the Bureau of Standards was well along on essentially the same design using conventional resistors and capacitors and a plastic oscillator block. Only handmade models of the loop-type fuze had been made but the design appeared sound both electrically and mechanically. An NDRC contract totaling $3,000,000 had been placed with Zenith for making production drawings of the Florida design, and for production of this design, first from temporary and then from production tooling. Development of production, inspection, and test methods suitable for large-scale production was also provided for. The Ordnance Department was already negotiating a contract with the Rudolph Wurlitzer Company for manufacture, starting from Bureau of Standards designs, of about 50,000 dipole fuzes with conventional resistors and condensers.

. At a later meeting the same day with General Hardy, production was tentatively allocated as follows: Zenith 200,000 per month; Globe-Union 100,000; and Wurlitzer 100,000. The respective Ordnance Districts were authorized to undertake contractual negotiations. This action, namely, purchase of about $60,000,000 of material for which no specifications existed, was probably unprecedented in Ordnance Department history. E. L. Moreland, Executive Officer of NDRC, was designated Chief Engineer of this program for the Chief of Ordnance.

Plans began to crystallize immediately. Within a few days, the Army Ground Forces had established a firm requirement for 1,500,000 trench mortar proximity fuzes to be supplied at a monthly rate of 400,000. Colonel Morton was appointed special assistant to General R. E. Hardy, Industrial Branch, Ordnance Department, to handle all Army problems relating to development and procurement of the fuzes. The Ordnance Department allocated $25,000,000 for the procurement of components, such as vacuum tubes, resistors, condensers, and alnico rotors. Meetings were held with fuze and component manufacturers to discuss procurement of components and expansion of component facilities.

The OSRD contractors, aside from component suppliers, who would be involved in the program were the Zenith Radio Corporation, Globe-Union, Inc., the State University of Iowa, the University of Florida, and the Raymond Engineering Laboratory; Ordnance contractors were the National Bureau of Standards, the Zenith Radio Corporation, the Rudolph Wurlitzer Company, the Rewelec Company (subsidiary of Globe-Union), Bowen and Company (contractor to the National Bureau of Standards), and Globe-Union, Inc.

In addition, two OSRD organizations, Division 4 and the Engineering and Transition Office, and two Service groups, Ordnance Department Industrial Branch and the Ordnance Department Research and Development Branch, were involved.

With so many agencies and groups it was a real problem to determine the respective responsibilities of each group. Definite understandings in this regard were mutually agreed upon and an informal steering committee was set up. Members of the committee were: Alexander Ellett, Division 4; Lyman J. Briggs, National Bureau of Standards; Colonel H. S. Morton and Colonel C. H. M. Roberts of the Ordnance Department. Its advisors were: Paul Scherer, OSRD, Engineering and Transi-

tion Office; Harry Diamond, National Bureau of Standards; Lieutenant Colonel P. L. Christensen and Major J. D. VanGeem of the Ordnance Department.

This committee co-ordinated the activities of the Ordnance Department and OSRD by promoting free discussion and free exchange of information. It met regularly throughout the program.

W. S. Hinman was appointed a Scientific Assistant on the Zenith and Globe-Union contracts and was given full authority to instruct them relative to their operations under their respective OSRD contracts. In addition, he was appointed an authorized representative of the Chicago Ordnance District Contracting Officer, giving him the same authority as regarded Ordnance contracts with Zenith and Globe-Union. These appointments insured both companies that all instructions, whether from OSRD or Ordnance, would reach them through one person, avoiding any possibility of conflict. The arrangement proved highly satisfactory.

By April, Globe-Union was producing about 10 fuzes per day on a model shop line. Designs were well along, temporary tooling was nearly complete, tooling for large-scale production had been started, parts for 20,000 fuzes were on order, and their factory production line was being set up.

Production of fuzes began on the production line in June. During June and July they produced some 100 to 150 fuzes per day on this line and, in addition, continued with the production of about 25 units per day on their model shop line. By the first of August the design was well stabilized, performance in the field was good, and the production line was operating smoothly. Fuzes delivered totaled about 8000. Arrangements were made to turn the facility over to Ordnance on August 12, but the close of the war was so imminent that the transfer was not made.

Zenith had, by the first of April, completed a preliminary design of the loop-type fuze and had started temporary tooling, which was essentially complete by May. Several small lots of fuzes, totaling about 30, were constructed and field-tested during June and July. By the end of July design details were complete, tooling for large-scale production had been started, and orders placed for 26,000 sets of parts.

As pioneers in the mortar fuze development, engineers at the Uni-

versity of Florida spent much time at Zenith and Globe-Union assisting engineers at those plants with design and production problems.

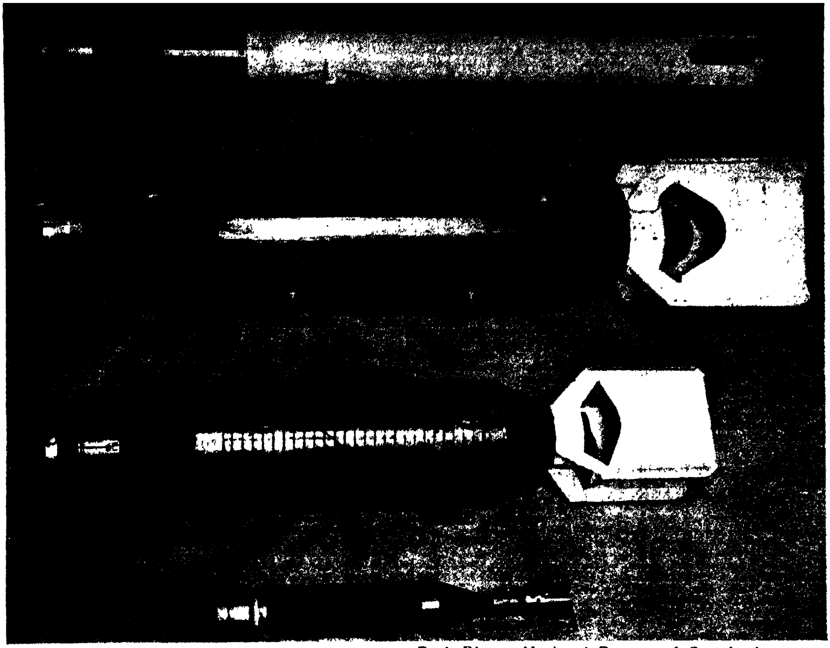
The State University of Iowa set up the proof-testing facilities and had just completed a model shop capable of producing about 100 fuzes per day when the war ended. In April 1945, the urgency of the trench mortar fuze program made essential prompt field testing. It was felt that the availability of a proof-testing area devoted primarily to the testing of mortar fuzes and located geographically close to the contractors would expedite the program greatly. The State University of Iowa, in co-operation with the U.S. Engineers Office at Rock Island, Illinois, located an ideal site on the Mississippi River near Clinton, Iowa. The University agreed to set up facilities and to operate the proving ground.

The station was situated on the Iowa side of the river just north of the Mississippi River Lock and Dam No. 13. This location offered a large area of flooded land over which the firing of mortar shells could be done without endangering river traffic. The whole area, including several promontories of land along the bank of the river, was under control of the U.S. Engineers and permission was obtained from them for its use. Construction work began late in April and by the first of June the installation, including both personnel and facilities, was substantially complete. These included an office building, laboratory, loading shed, storage buildings, observation towers, recording equipment, etc., and a staff of about 30 people. The speed with which the operation got underway was remarkable and was due in a large measure to the efforts of L. L. Friez, Director of the work at the University of Iowa. The Army co-operated wholeheartedly in supplying promptly special equipment such as telescopes, aiming circles, mortars, shells, loaded booster cups, etc.

The proving ground was capable of handling about 100 rounds per day, including computation and analysis of data. The work of developing and reading films, computing and analyzing data, writing reports, and handling business details was done in an office building located in the north section of Clinton, three miles from the field station. In most instances a complete report on a firing program was sent out within four hours after the raw data were received from the field. When the war ended the proving ground was abandoned.

All contracts, which had been initiated against the contingency

that war in the Pacific would last until mid-1946, or early 1947, were abruptly canceled after August 1945. The story of Division 4 ends at this point, but direct arrangements were made between the Ordnance Department and the National Bureau of Standards to continue a fuze-development program at the Bureau.

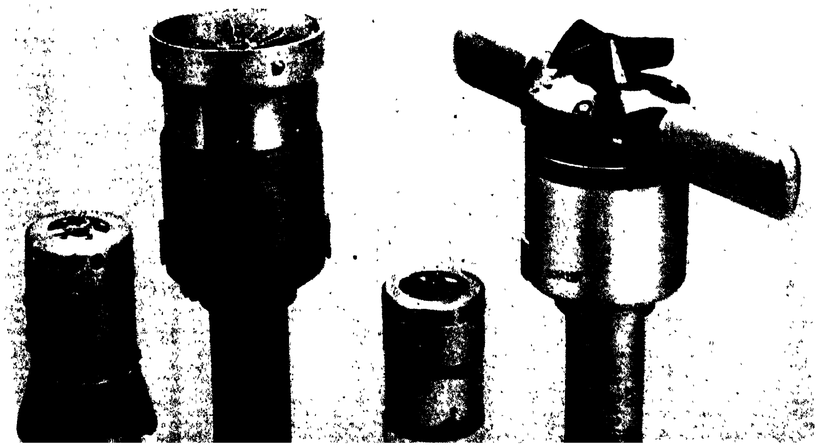


Both Photos National Bureau of Standards

Radio proximity fuzes

ABOVE, TOP TO BOTTOM: *5-inch high-velocity, aircraft-fired rocket with ring-type rocket fuze; 500-lb. general-purpose bomb with bar-type bomb fuze; 260-lb. fragmentation bomb with ring-type bomb fuze; 81-mm. mortar shell*

BELOW, LEFT TO RIGHT: *rocket fuze, ring-type bomb fuze, mortar fuze, and bar-type bomb fuze*



Official Photo U. S. Navy

Preparing for a field test of Pelican, a radar-homing glide bomb

CHAPTER XXIV

GLIDE BOMBS: PELICAN AND BAT

THE ACTIVITIES of NDRC in guided missiles started in three independent projects divided between two of its early administrative Divisions. The first of these, a glide bomb, originated in Division A (Ordnance). Originally conceived as a bomb-carrying glider controlled by television, it later became Pelican and finally reached some combat use as Bat. These two code names represented two varieties of a radar-homing bomb. The second type originated in the instrument section (D-3) of Division D. Again conceived largely as a television weapon it saw considerable combat use as Azon, a bomb of nearly conventional size, visually observed and radio-controlled in one dimension. It was particularly effective against long, narrow targets such as bridges. Later versions, one Razon, radio-controlled in two dimensions, and the other Felix, a target-seeking bomb, had reached acceptance tests as the war ended. The third project, named for the mythological Roc, originated in the radar section (D-1) of Division D. It ultimately turned out to be unsuitable for radar control but well adapted for use with television. Development tests for that application were well advanced as hostilities ended.

Among the several variables — size, maneuverability, method of control, and tactical use — a number of classification schemes are possible. The picture was not always clear. Sometimes a code word represented a missile, sometimes it represented a method of control. In theory the various missiles might each be subject to a variety of methods of control. In practice there are many restrictions, as the changed applications of the original projects have already testified.

Perhaps one variable is more nearly fundamental than the others, namely the maneuverability of the missile. It controls the use of the missile directly by dictating the circumstances of its launching and indirectly by its stowage requirements. The distinction between glide-angle and high-angle missile has frequently been made but is rather superficial. A highly maneuverable bomb can be used at a glide angle

If its structure is strong it can, theoretically at least, be used at a high angle. A less maneuverable bomb must be used at a higher angle.

The Azon-Razon bomb falls essentially in a ballistic trajectory which can be corrected by an amount which is only a small fraction of the height of fall. Pelican has wings and develops enough lift to carry it a distance several times the height of release. Intermediate is the Roc, which begins to fall in a ballistic trajectory but possesses aerodynamic devices capable of deflecting it through a distance commensurable with the height. Such agility makes an accurate bombsight approach unnecessary.

After these diverse origins the several guided-missile projects were brought under a single administrative control at the time of the general reorganization of NDRC late in 1942. H. B. Richmond was Chief of Division 5 until January 1, 1945, when he was succeeded by H. H. Spencer, earlier Deputy Chief and sometime Technical Aide of the Division. The members of the Division were the Section Chiefs named below, together with F. L. Hovde, and L. N. Ridenour (later succeeded by A. L. Loomis). Buckley remained a member of the Division after relinquishing the position as Chief of Section 5.3.

Because there were many problems common to these several projects the new Division 5 was organized somewhat upon a systems-and-components basis. The major projects, glide bombs and the so-called high-angle bombs, were assigned to Sections 5.1 and 5.2 under their respective Chiefs, H. L. Dryden and L. O. Grondahl. They were known as the Washington and Pittsburgh Projects. Television activities, for whatever missile, were centered in Section 5.3 under O. E. Buckley, who was later succeeded as Chief by Pierre Mertz. J. C. Hunsaker was Chief of the aerodynamic section, 5.4, which took charge of the Roc project. He also provided all projects with liaison with the National Advisory Committee for Aeronautics of which he was chairman. A number of problems of servomechanisms, stabilization, and control were handled by Section 5.5 whose Chief was J. C. Boyce.

Guided missiles, from the point of view of method of control, may be grouped in two classes. Those in the first class, and Azon is an example, are under the control of the operator throughout their flight. In the second class belong those missiles which are automatic in opera-

tion, "homing" on some source of inherent or reflected energy by which the target can be distinguished from its background.

For the first class, the most popular type of control has been by means of a radio link, although the Germans experimented successfully with a direct wire link. Other methods of control, such as heat, light, and sound, have been proposed and tried. In the second class, inventors of all kinds, from the kitchen mechanic to the large industrial laboratories, have considered numerous sources of usable intelligence for homing.

In 1888, Heinrich Hertz demonstrated the existence of electromagnetic waves, now known as radio waves. By 1895, Dueretet and Popoff had succeeded in transmitting wireless telegraph messages between the shore and a Russian battleship. Two years later, in 1897, Ernest Wilson controlled a torpedo in the Thames River by Hertzian waves. Lieutenant Bradley Fiske, U.S.N., filed a patent application in 1898 on means for controlling torpedoes by wireless.

With the advent of the airplane it required only a simple transfer of ideas to bring forth the radio-controlled aerial torpedo. John Hays Hammond, Jr., was the American pioneer in successful application of radio control to torpedoes, ships, planes, and other aerial vehicles. A patent application filed by him on March 7, 1914, for example, claims "a system for controlling an airplane or aircraft at a distance by radiant energy," using an automatic pilot which is overriden by the radio commands.

Upon the entry of the United States into the first World War, activities of our Army and Navy in this field were greatly stimulated. On independent and highly secret projects, Hammond and Charles F. Kettering worked for the Army, and Hammond and Elmer Sperry for the Navy. Kettering's pilotless aerial torpedo was not remotely controlled, but its construction brought into focus many of the stabilization problems which were to plague all who entered the field later. Kettering's interest, thus aroused, was to be revived in 1938 when a second war appeared imminent. Hammond built radio-controlled airplanes and torpedoes. Sperry designed radio-controlled airplanes and a crude dirigible bomb. This last, never completely tested, fell suspended from a sort of parachute, steering being accomplished by tilting the parachute. Well-meaning individuals were to rediscover this idea

independently as much as twenty years after it was patented by Sperry, and to wonder why their suggestions were brushed aside.

Guided missiles were discussed in the early planning of Division A and, at its second meeting, by NDRC itself. In August 1940 RCA broached to Richard C. Tolman, Chairman of Division A, its idea of a television-equipped radio-controlled aerial torpedo. RCA felt competent to undertake the television development but was not equipped to investigate the aerodynamic aspects. NDRC agreed that the proposal was sound and in January 1941 work was started under Section A-E on both segments of the work. RCA agreed to develop suitable television equipment, and Hugh L. Dryden, of the Bureau of Standards, was appointed consultant on the aerodynamics. Dryden had published in 1927 the fundamental report on *Aerodynamics of Aircraft Bombs.*

As was the case with much of the NDRC work before the date of Pearl Harbor, this project was operated at first on a small scale with only a few workers. Progress was correspondingly slow. RCA studied improvement of the sensitivity of the small lightweight television set they had already developed as suitable for remotely controlled planes, gliders, and missiles. They succeeded in producing a smaller iconoscope which used electron multiplication. This gave about eight times the sensitivity previously available, which was good, but still less than the goal RCA had set for itself.

In the meantime, an aircraft corporation had been granted a subcontract by RCA to develop the bomb carrier and had built a model which was aerodynamically unsatisfactory. The vehicle project was therefore taken over by the Bureau of Standards group under Dryden. Vidal Research Corporation contracted to do the actual building of the air frames. This transformation of Dryden from a consultant to the active director of the project was one of the prime reasons for the later success of the NDRC glide bomb. By designing the bird and its controls as an integrated unit in accordance with data obtained under his own supervision he was to produce a missile which eventually proved effective in combat. The Services expended a lot of time and effort upon too many separate projects for glide bombs of various types. Most of these proved disappointing. One of the reasons was this very lack of integration that Dryden had shown to be necessary.

The first flight tests were made on a glider with an 8-foot wing

span, built as designed but to a 0.7-scale of the 12-foot bird. Later it was to occur to Captain D. P. Tucker and Lieutenant Commander G. W. J. Dan Junas, of the Bureau of Ordnance, that this 8-foot model might be useful in its own right. The summer of 1942 marked the peak of the submarine menace to our shipping, with daily reports of sinkings in sight of our own coasts. These officers visualized a radar-homing missile as one weapon usable at night when the surfaced submarine is most vulnerable. It would consist of an 8-foot Bureau of Standards glider, a radar-homing mechanism then under development at M.I.T., and a standard 325-lb. depth charge. With this beginning, radar became an important factor in the project.

The television-guided glide bomb, designated Robin, was built and tested in both 0.7-scale and 12-foot models. All the mechanism worked moderately well, but errors averaged more than 600 feet in the final drops. Part of this error is inherent. At the glide angle used, about eleven degrees from the horizontal, the target is seen greatly foreshortened. An angle of 10 mils is subtended (from 15,000 ft. altitude) by 15 feet in azimuth or by 399 feet in range. A slight error in steering in the range co-ordinate must therefore introduce a large error in the drop.

By this time the Pelican, using radar, was showing itself capable of conforming more closely to the desired flight path than its counterpart could using television with manual guidance. As a result, Service interest in the television glide bomb faded, and this activity lapsed into desuetude. Instead, the Division devoted the time of its television section to work in connection with Roc, which showed more promise. All the remaining work the Division undertook in the glide-bomb field dealt with the radar-homing missile.

A radar-homing device has three possible sources of information. It may carry its own transmitter, a friendly transmitter usually located on the dropping plane may be used, or an enemy radar transmitter may be the target. Some work was done along each of these lines, the first called Bat, the second Pelican, and the third Moth.

To eliminate one of the variables in the problem, the first flight tests of the radar-homing glider were carried on against a beacon target as if the missile *were* a Moth. Operational details were to rule out Moth as a combat bird, but the beacon as a source of energy remained active as a test device throughout the project. Pelican development was

initially somewhat in advance of Bat, largely because of the relative progress on the two radio sets involved.

The M.I.T. Radiation Laboratory produced the radar for this work. The first was equipment for the Radar-Homing Bomb (RHB). This required a separate transmitter not expended with the bomb. For operational use, of course, location of the airplane carrying the transmitter was governed by the requirement that angles be such that the target be distinguishable from the background by means of reflected radar pulses. The second, to come along later, carried transmitter and receiver in one unit (SRB—Send-Receive Bomb). The whole bird therefore became independent of its parent at release.

After the reorganization of NDRC in December 1942 the radar work was continued at the M.I.T. Radiation Laboratory under Division 14. Dryden's work at the Bureau of Standards was supported by Section 5.1, Dryden, Boyce, and Mertz. In its turn, and in evidence of strong Navy interest, the Bureau of Ordnance established an Experimental Unit at the Bureau of Standards and a Field Test Unit at Warren Grove, New Jersey. It is difficult to unravel the chain of command, but it may be said that co-operation between NDRC and Navy was extremely close. Results were the main objective and credit was neglected. Roughly the experimental work, design, and testing were under Dryden, while assembly and mechanical assistance in field tests were provided by the Navy.

In 25 flight tests during the winter and spring of 1943, first against a target beacon and later against a reflector suspended from a barrage balloon, good homing was observed and attempts were made to determine the optimum adjustments of variable factors in the radar and servo equipment.

By September, with improved instrumentation and correspondingly greater attention to detail, 35 additional flights had permitted optimism to enter the reports. It will appear in other sections, notably in connection with Roc and Azon, that it was only when complete and accurate instrumentation made it possible both to determine the causes of failure and to measure the effects of variation of design parameters that progress became more than sporadic. From this time on the glide-bomb program moved ahead, with all concerned confident of eventual achievement of success.

The Radiation Laboratory was satisfied that the radar equipment was perfected, and turned its attention to other fields. In its turn the Navy decided to organize a few squadrons to use the missile in combat. But these developments did not indicate complete confidence that Pelican was ready for service. There was still work to be done. There were still troubles that had to be eliminated. They were not troubles that vitiated the theory — they might even have seemed trivial in the laboratory — but they were numerous and maddening in the field. Engineering choices of components, construction, and layouts needed modification to eliminate bugs.

One thing that almost insured additional headaches for the trouble-shooters arose from the fact that the bird had to be redesigned in larger size. The submarine threat had faded perceptibly, as other weapons proved adequate to cope with it. The demand for Pelican for this purpose therefore vanished; instead a bird had to be designed to carry the standard 500-lb. bomb. And of course every new design is more or less experimental, calling for bug-chasing all over again.

Nevertheless, by the end of 1943, the independent groups which had contributed to Pelican felt that development was over and that production engineering was justified. The radar set, the air frame, and the controls were all put in production, and all that seemed necessary was to fuze them into an operating weapon. As a nucleus for a new research group, early in 1944 five men were transferred from the Radiation Laboratory to a Field Experiment Station established by M.I.T. at the Bureau of Standards. Ralph Lamm, project engineer, was in charge, assisted by Perry Stout. This group operated under a contract from Division 5 of NDRC, which brought the radar work under the same administrative control as the other portions of the project. The new group was intended to serve two functions, project engineering and pure research. It transpired that what was needed was not only trouble-shooting but also basic information upon which to make engineering recommendations.

A Liberty ship, the *James Longstreet,* having been made available for tests, six standard production-model missiles were dropped in June 1944. To the chagrin of all concerned, all six failed to home on the target. Ultimately, investigation was to prove that the causes of failure were minor in character and easily corrected, but this was not known at the

time. Perry Stout has characterized this point as the end of the "I seen it" phase. Quantitative measurement of all details of performance was substituted for the combination of intuition, uncorroborated observations of oscilloscopes, and measurements, which had served as the basis for earlier engineering decisions. Credit must be given to Perry Stout and Edward Toporeck, who made many of the measurements needed for assessment of performance of components and completed mechanisms.

Combining the failure of this batch of Pelicans with the expectation of early perfection of Bats, the Navy concluded that it had no prospective operational use for Pelican and restored the project to a developmental status. The expected advantages of Bat arose from the fact that it was self-contained, requiring no further predetermined maneuver by the dropping plane after release. With Pelican, of course, the mother plane had been required to keep the target illuminated by a radar beam. This in turn entailed precise control by the radar operator. Pelican had an effective possible range about twice as great as Bat, was lighter, carried simpler equipment to be expended, was not so greatly hampered by increase in signal strength as it neared the target, and above all was much nearer readiness for combat. The responsible officers of the Navy, weighing these factors, preferred to wait for Bat. The reasons will be discussed later.

Using the same type air frame as Pelican, Bat was designed as a 10-foot glider. The Navy acting independently of NDRC had placed a contract with Bell Telephone Laboratories for the design of an SRB homing system. As usual, flight tests of the first models showed the need for changes in design. When 11 Bats were dropped against the *James Longstreet,* it was noted that there were only moderately satisfactory results.

Some of the difficulties were inherent and had been anticipated; others were not. It had been realized that all these birds hunted — oscillated about the average flight path — and the accuracy of the hit depended upon the stage of the hunt cycle at which impact occurred. A parallel project for the reduction of hunting had already been undertaken by the M.I.T. Servomechanisms Laboratory for Pelican and was continued for Bat. Gordon Brown of that laboratory agreed in February 1943 to study the entire problem of the relationship between the characteristics

of the servomechanism and the flight characteristics of the glider. The project was directly under A. C. Hall.

Hunting is of course intrinsic in homing unless eliminated by some special device or design feature, although the speed and magnitude of this oscillation are subject to control. If the bird senses an error in heading and endeavors to correct it, an unavoidable time lag occurs in execution of the commands. It takes some time to sense the error, more time for the control surfaces to move to the new position, and still more time for the controls to swing the bird around to the new heading. By that time there will usually be built up a velocity which can carry the swing on past the correct heading. The controls will have to reverse, stop the overshoot, and then return the bird toward the proper course.

The amount of hunting can usually be controlled by a human operator by anticipating the arrival of an error signal and acting upon that anticipation. In an automatic mechanism what is needed is a device (in one case a rate gyro) to sense the rate at which the error angle is changing. By choosing the proper constants, hunting can be minimized by a control mechanism in which the rudder angle is a linear function of a combination of the error angle and the rate of change of the error angle.

Hall and his assistants built a test table which helped greatly to determine the proper values of the pertinent parameters. Tilt and rotation of the table were so adjusted as to match in magnitude and velocity the corresponding values for the bird. Without expenditure of material hundreds of flights could be simulated within a relatively short time. With the aid of this table, two different systems for stabilization and control were designed and built. The table and related simulative equipment were used to determine the optimum adjustment of the earlier system developed at the Bureau of Standards. Both systems were given field tests at Warren Grove, New Jersey, and Manteo, North Carolina. The revised model of the control equipment was engineered for production in co-operation with Bell Telephone Laboratories, but did not appear in time to reach combat.

Some other sources of trouble which were inherent in Bat could be minimized but not eliminated. In co-operation with the Bureau of Ordnance, the M.I.T. Field Experiment Station attacked these problems. They devised a large number of ingenious tricks and succeeded in

making Bat a useful weapon, but it was a race against time. Up to the very last minute, modifications were necessary in the combat model to provide for the latest developments in this field.

By early spring of 1945, there was sufficient confidence in Bat for the Navy to send a squadron of Bat-equipped Privateers to the Pacific for combat tests. Stout and McCoy went along as Technical Observers, their duties ranging from training operators to designing targets for demonstrating Bat's effectiveness to various Admirals. As civilians they were not eligible to participate in combat missions, but they found themselves in constant demand both at the rear and at the forward bases for consultation concerning details of adjustment and operation.

It was evident even to the Japanese that something special was brewing when the squadron arrived on Palawan. The Bats of course had to be slung outside the carrying plane, one hung under each wing, and that in itself was a tip-off. What was afoot turned out to be the first, and so far the only, use of a completely automatic homing aerial bomb in combat.

At intervals from May until August 1945 a number of Bats were released experimentally against the enemy. As Japanese shipping was disorganized by that time, the only available targets were small ships, usually in or near harbors. In spite of the increased difficulty this situation presented to the operators, good hits were obtained.

From distances of several miles, well beyond effective antiaircraft fire, the Bats followed moving targets relentlessly until destruction was accomplished. The most spectacular of the hits occurred when an ammunition-laden picket boat vanished in one terrific blast and when a hapless destroyer had its bow blown off.

Now that Bat has proved its value, it may be instructive to review the reasons for rejecting Pelican in favor of the later weapon. It will be remembered that in the summer of 1944 Navy authorities concluded that they had no operational use for Pelican and that the efforts of all concerned should be devoted to development of Bat. This decision came shortly after the unsuccessful tests against the *Longstreet*. But in concluding that Bat would work better, the fact was glossed over that the same type of air frame, servomechanism, and radar receiver was used in each bird, while in Bat there was the additional complication of an untried transmitter and several months' delay in readiness for combat.

Most of the representatives of Division 5 and its contractors feel that had the same amount of energy been expended in grooming Pelican for service as proved necessary with Bat, the former would have shown itself the better weapon.

One important factor in the decision was the strict Navy requirement that targets be positively identified before attack. This had been necessary to avoid complications in European waters but was perhaps not so important in the vicinity of Japan. Two of the advantages of Pelican, that it could attack from distances of about twenty miles, and that it could be used through overcast or cloud cover, were nullified by this restriction.

Another advantage of Pelican arose from the operation of a natural law. With Pelican the transmitter remained in the vicinity of the release point, while with Bat of course the transmitter approached the target. The result was that in the former case the signal strength at the receiver varied approximately as the inverse square of the distance from the bird to the target, while in the latter it varied inversely with the fourth power of the same distance. Literally months of frantic effort on the part of the M.I.T. Washington radar group were necessary to make Bat operable in the face of this tremendous increase in signal strength as Bat neared its target. Last minute information is needed for the final path corrections which would give it accuracy. Obviously the same law cited above insures that Pelican is less troubled by this effect. Division 5 dissented strongly from the decision rejecting Pelican, and still feels that it was unwise. Nevertheless, of course, it acted none the less vigorously in assisting to make Bat an operating success.

Bat was the product of many hands and minds. Of the groups working, Division 5 sponsored the Bureau of Standards organization, which built and tested the air frames and servomechanisms, the M.I.T. Servomechanisms Laboratory, and the M.I.T. Field Experiment Station. Emphasis in this history on the part Division 5 contractors played in the Pelican-Bat project is not intended to minimize the large responsibility borne by the Navy and Bell Laboratories. The results obtained can amply sustain credit for all concerned.

CHAPTER XXV

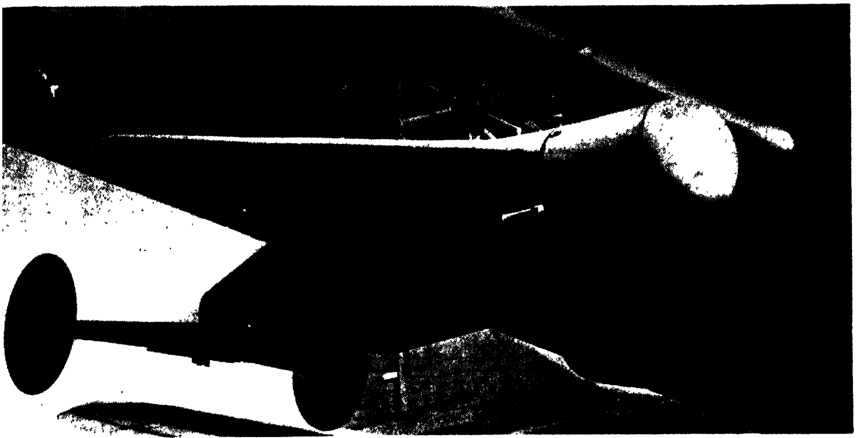
THE ROC

ROC was named for the mythical bird which sank Sindbad's ship with a boulder dropped from its talons. This project, while it never culminated in combat use, was one of the major activities of the Division.

In November 1941, the M.I.T. Radiation Laboratory had already developed some of the many radar devices which were to carry its stamp into all combat theaters. It occurred to E. L. Bowles, then Secretary and member of Section D-1 (and later Expert Consultant to the Secretary of War), that radar could be used in guiding a bomb. The Radiation Laboratory had a radar system, AGL, which seemed capable of modification to fill the bill. The main thing that was necessary was to design a flying bomb which could be made to fly down the radar beam. The Douglas Aircraft Corporation agreed to undertake the design of such a bird, and the project was started with a modest appropriation of $30,000. During the next four years, nearly two and one-half million dollars were to be needed to carry the work through all the vicissitudes which were to appear. The initial arrangement left responsibility for the radar in the capable hands of the Radiation Laboratory, for the aerodynamics in the equally capable hands of Douglas, with co-ordination under Frank Collbohm of Douglas and L. N. Ridenour and David Griggs of the Radiation Laboratory.

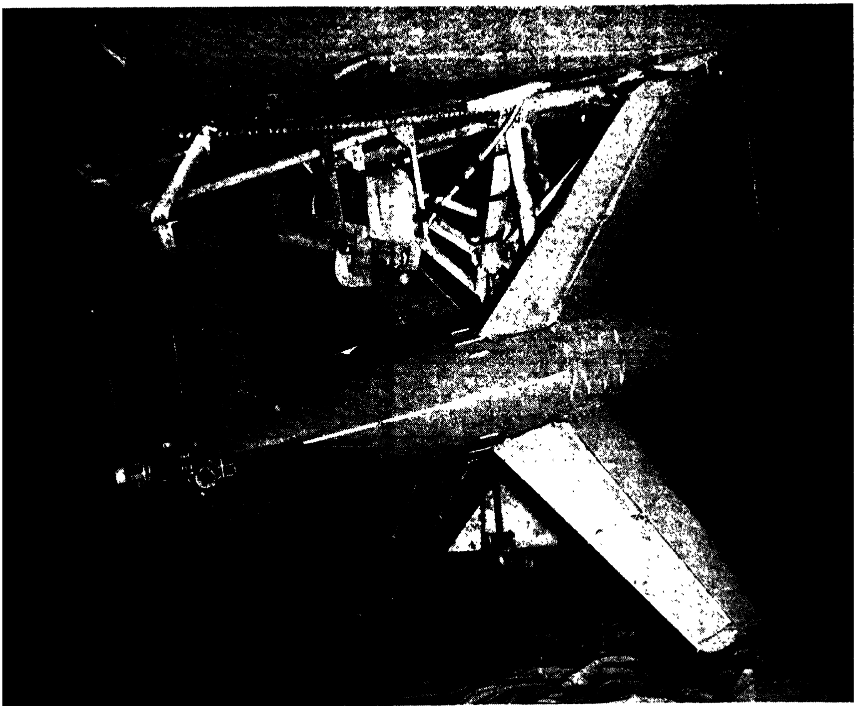
Preliminary design work was undertaken by W. B. Klemperer, who had come to Douglas from the Goodyear-Zeppelin Corporation. One of his early contributions had been an esoteric study of pursuit curves, a study pertinent to the Roc problem, but primarily applied to the interception of bombing planes. It was the philosophy expounded in this study which is exemplified in the latest Douglas bomber, the XB-42, familiarly known as the "Mixmaster."

The need for sufficient agility in Roc, combined with the glide angle of approximately 45° that was desired, necessitated wings to provide adequate lift. Wind-tunnel tests proved the practicability of an X-wing



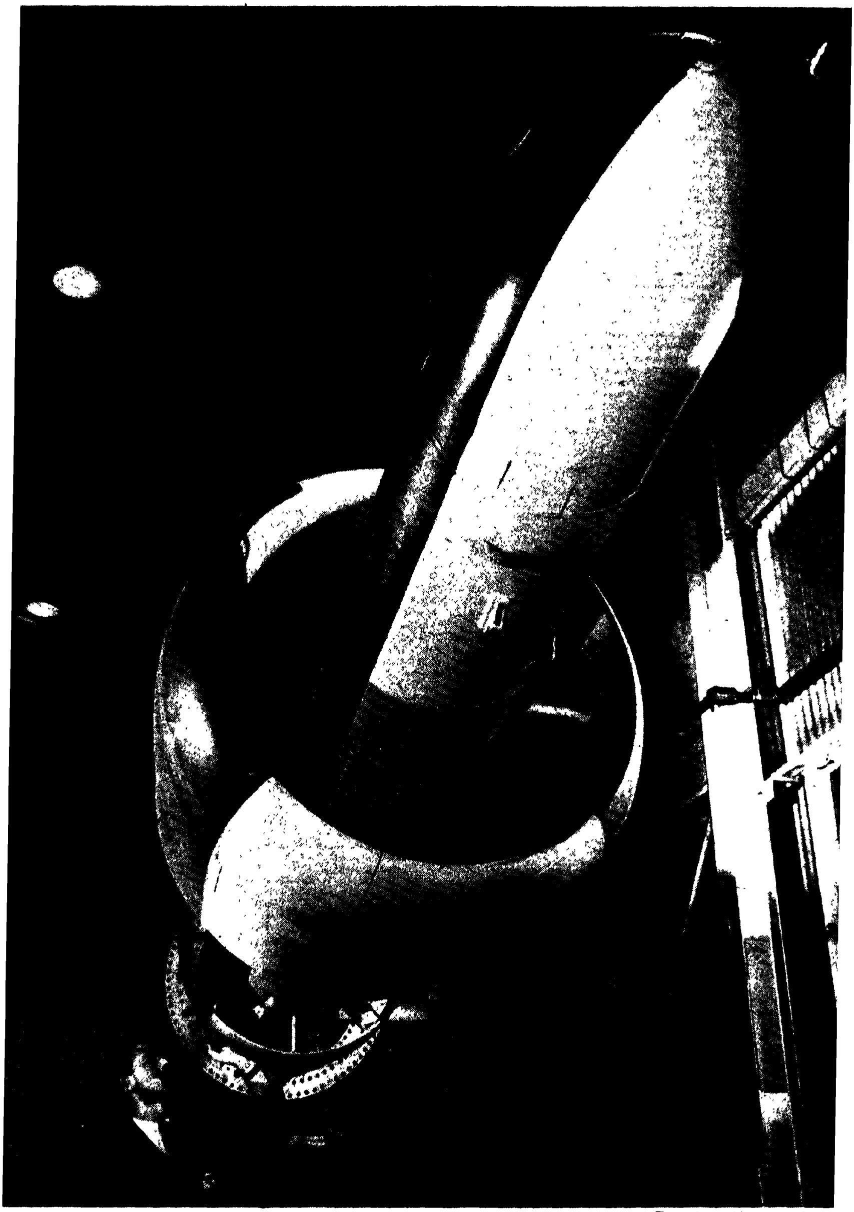
Official Photo U. S. Navy

Bat, self-contained automatic radar-homing glide bomb, suspended from the wing of a Navy plane, ready for action



Douglas Aircraft Co.

The first model of Roc suspended from the wing of a Flying Fortress



Douglas Aircraft Co.

The redesigned Roc, with a television camera in the nose

model, such that lift could be generated in any direction normal to the bomb axis without having to bank.

Two features of the new bird were distinctive. It turned without banking, and it flew at zero angle of attack. This meant that it always looked where it was going — its axis was along the tangent to the trajectory. It required careful attention to aerodynamic forces to permit such a design, and in the end zero angle of attack proved a goal rather than absolute reality. Nevertheless the facts came very close to the dreams.

The first Rocs were designed to carry a 500-lb. warhead in a streamlined shell equipped with four wings and four interdigitated tail surfaces. A gyroscope operated wing flaps as ailerons to keep the rate of roll of the bomb within accepted low limits. Flight was also controlled by the wing flaps. The very first plan, to have the bird fly down a radar beam, was very shortly changed to make it a radar-homing bird. A variety of reasons necessitated this change, among them being the greater freedom of action allowed the carrying plane.

This decision, however, introduced new problems. There was some fear that at the angle of fall of the new missile, intermediate between the high-angle and the glide bomb, the target reflection might not be distinguishable from the background signal. This was to be determined by experiment. In any event, the radar problem was the responsibility of Griggs and his assistants at the Radiation Laboratory, and Douglas was still concerned with problems of design of the bird itself.

About this time occurred the reorganization of NDRC. Section 5.4, consisting of J. C. Hunsaker and H. L. Dryden, with H. H. Spencer, Technical Aide, took over supervision of Roc. The presence of L. N. Ridenour, and later A. L. Loomis, as members of the Division provided liaison with the radar work.

When the first models of the air frame were ready for drop tests, the radar equipment was not available in expendable quantities. Plans were therefore made to use a photoelectric homing eye as an interim testing device. In this way one variable would be eliminated — the photoelectric cell used at night with a flare as a target was known to be foolproof. (It turned out that the photoelectric cell was also in need of development.)

A four-quadrant photoelectric cell was used for locating the target

direction. This required some development work to get a high-quality cell. The glass blowing required to give a satisfactory cell proved too much of a problem for the first contractor selected. Farnsworth, with a background of experience in building television receivers, agreed to undertake the task in spite of a very crowded production schedule and did a very satisfactory job. Thanks for this service are due to O. S. Duffendack, Chief of Section 16.4, who granted release of Farnsworth shop time. The first nine light-seeking Douglas birds were equipped with Farnsworth cells. Extensive laboratory, wind-tunnel, ground, and airborne tests preceded the actual drops.

Of the nine birds dropped, four homed to the satisfaction of the test group, and five failed due to malfunction of some part. There was therefore additional information to be gained by continued drops. Farnsworth, however, found that they could not continue to overload their research shop with the making of additional photo cells.

In the emergency, Section 5.5 offered to have a new concern develop a photoelectric scanner which would be easier to make and use. Henry Blackstone and Curtis Hillyer had been working for three years on a photoelectric control for various missiles. Originally it had been devised to steer antiaircraft projectiles. Later, under the sponsorship of the Fairchild Camera and Instrument Company, it was applied to glide bombs for Wright Field. It was relatively simple to transform it into a control for Roc.

Fairchild designed and built six photoelectric eyes for Roc and helped to install them. These eyes used a light-interrupter in front of a standard photoelectric cell. With their aid further information was obtained of value in refining operation of Roc.

For the entire series of Douglas birds, there was needed an intermediate amplifier and decoder to take the information from the target-seeking eye and translate it into commands to the control surfaces. This was definitely an electronic job requiring considerable development work. Section 5.5 therefore arranged a contract for this work with the Pacific Division of Bendix Aviation Corporation.

Models of the Bendix equipment were to be made and delivered in time for each of the different versions of Roc — two for photoelectric eyes, one for the radar-homing, and one for the radio-controlled bird. For the latter, W. S. Leitch developed two different systems to allow

transmission over the radio link of signals to indicate the amount of rudder motion desired. This will appear later.

All this work required constant collaboration with Douglas engineers, and the record shows that this was accomplished with credit to all concerned.

By December 1943, most of the design problems had apparently been settled, and Douglas was instructed to design an X-Roc (crossed wings) suitable to carry the 1000-lb. GP bomb and the Zenith RHB radar receiver. The M.I.T. Field Experiment Station in Washington was assigned the job of modifying the radar set (if necessary) to make it work in Roc. The two questions unsettled were inherent in the fact that RHB had been designed for glide bombs, with a lower angle of approach. First, it was necessary to decide whether to keep the eye blind or to bias it during the initial curved portion of the trajectory. Second, it was still unknown whether sufficient echo contrast could be developed between the target and its background at the high angle of approach assumed by Roc. This problem has been mentioned previously.

While this really fundamental question remained unsolved, development continued on the bird. In April 1944, the Navy reported that they would have no interest in Roc unless it could be transported on carrier-based aircraft. Since the X-Roc could be suspended on only the larger bombers, and not on any carrier-based planes, this necessitated complete redesign of the bird.

With expansion of the project from a small research problem to an engineering and production task, Douglas organized a Special Project Group in their Santa Monica Engineering Laboratory. Klemperer remained in charge of research while Kingdon Kerr was appointed Project Engineer. Elmer P. Wheaton became co-ordinator, spending half time in the M.I.T. Radiation Laboratory and the rest in Santa Monica. This arrangement was to last through 1944, when the Douglas Applied Physics Laboratory took charge of operations. Under competent direction, design and production proceeded rapidly.

In the new model, Roc OO-1000, the crossed wings were replaced by a ring type of shroud, which could be rocked on a universal joint to create lift in any direction. With the fixed tail also built as a ring shroud, the new version merited its nickname, the "Double Cookie Cutter."

Shortly after the new bird was ready for tests, authoritative word came from the M.I.T. radar group that the long-awaited studies on target contrast at the desired glide angle had been completed. The answer was disconcerting: reliable homing signals could not be obtained at that angle. Here, then, was a bird of exceptional capability but its excuse for existence had been withdrawn. It will be recalled that the project had arisen from the idea that radar was about ready, and Douglas had been asked to design the ideal bird to use radar. They had responded by designing a bird which possessed stability, ability to home, and unusually high aerobatic maneuverability. Two other applications of Roc were also considered. The first was the direct-sight radio-controlled bird; the second a homing Roc.

The direct-sight bird, a sort of super-Razon, was proposed for use against difficult targets where the great available amount of flight-path correction could be utilized. Division 7 proposed, and Schwien built, some versions of the Carp bombsight attachment which was to provide the preliminary control according to a fixed program. By first sailing the bomb toward the target, then diving it in, all in accordance with a calculated schedule, the bird could be brought into collinearity with the line of sight to the target. During the last few seconds, the eclipse method of control could therefore be used. If the bomb is maintained so that it continuously eclipses the target from the bombardier's view, it must necessarily score a hit regardless of any errors in the dropping technique. This procedure had been considered for Razon but abandoned because of insufficient maneuverability. Roc was freed from the requirement that the bomb fit into the bomb bay, and its larger control surfaces correspondingly permitted much greater maneuverability.

Unfortunately, experience proved that theory can be right but impractical. In a disheartening series of tests it appeared that the combination of a complicated additional sighting mechanism with an extremely athletic bird was just too much for the operator to handle. The sponsor of the scheme was unable to perform all the operations required of the bombardier, so several wild flights ensued with poor scores on the target, and the project was abandoned.

The other scheme proposed for Roc involved fitting it with a television eye and controlling it from a shepherd plane or even from the ground. The story of television for guided missiles is quite checkered,

and will be found in three sections in this history. The Hazeltine, Farnsworth, and Remington-Rand contracts were sponsored by Section D-3 for the high-angle bomb, and are discussed along with Azon. The RCA work arose in Section A-E for use in a glide bomb and has been mentioned under Pelican. After abandonment of the immediate plans for use of television on the two vehicles indicated, Section 5.3 continued work in this field, partly as a help to the Services in connection with Army glide bombs and Navy Drones, but primarily for use in Roc.

Section 5.3 performed for television a service somewhat analogous to that by Division 5 for guided missiles in general. We have seen that originally two different sections supervised development work on television for bombs. As the complexity of the work grew it was necessary to co-ordinate the work under one director. O. E. Buckley of the Bell Telephone Laboratories became head of Section 5.3 with such authority. He was assisted by Pierre Mertz and the other members of the Section, O. B. Hansen and D. E. Harnett.

Under various contracts with RCA, a large number of engineers had been put to work on all aspects of the television problem. The main features covered improvement of picture quality, sensitivity, reliability, and selectivity, design of antenna, and reduction of size and weight.

By the fall of 1943 there were available proposed television sets from three different manufacturers, each having its own peculiar advantages. It was necessary to decide which of these features merited adoption, and no amount of separate observation was capable of solving the problem. A contract with the Columbia Broadcasting System Engineering Research Laboratory was therefore arranged under which competing equipment was given comparative tests under controlled conditions. Demonstrations of the relative merits of the different cameras were visited by a large number of Army, Navy, and NDRC representatives.

As a result of these tests, it was agreed that the RCA image orthicon seemed best suited to military requirements. These requirements, it might be mentioned, involving simplicity and reliability under extremes of battle conditions, differ widely from civilian needs. RCA promised to have ready for test a few models of an improved Miniature IMage Orthicon (MIMO) by the end of 1944, and reasonable quantity production a year or so thereafter. MIMO was therefore adopted as the intelligence unit for Roc.

Razon and Azon had been designed with a simple radio link. On-off control had been proved adequate, although a proportional control scheme had originally been thought necessary and used in early tests. Roc, however, responded more enthusiastically to slight shadings of control, and it was determined that proportional control was necessary. This means that the setting of the control surface on the bird should correspond to the position of the operator's control stick. As has been mentioned, Bendix undertook to devise a proportional-control system which entailed a minimum of alteration of existing standardized equipment.

The first plan, known as DAM (Differential Amplitude Modulation) proved unsatisfactory in field tests. In it the four standard audio-control tones from a regular RC-186 Bendix transmitter were used simultaneously, with rigid automatic volume control on the output. The control stick, in moving from full right to full left varied the amplitude modulation of the right-left tones from 100% right through 50% right, 50% left at the neutral point to 100% left. In intermediate positions the relative degree of modulation of the two tones, always adding up to 100%, determined the position of the rudder in accordance with the stick position. This system worked well in the laboratory but showed some instability at the neutral point in field tests.

A modification, Proportional Modulation (PM) was adopted and gave good performance. It was similar to DAM, but merely used one tone at a time for each co-ordinate, the percentage modulation varying from zero at the neutral point to 100 at the extreme position of control stick and rudder.

Most of those concerned with the project had felt that, given a good television image, flying the bomb should be virtually the same as flying an airplane to a given spot. Even this latter feat is not so simple as it might seem, as demonstrated by the large number of kamikaze planes which missed through pilot errors. Flying a plane or glider by television control demands even more of the operator. As one pilot has phrased it, "The seat-of-the-pants reaction is missing."

Division 7 mentions in its portion of this volume the laboratory studies that were made by Loebe Julie at Columbia University with a simulator for the MIMO Roc. With this preliminary device operating in one co-ordinate it was evident that control could be accomplished, but

good results necessitated a trained operator. Training could obviously not be done in the field using very expensive and scarce birds. (Only ten MIMO sets were available by the time the project terminated.) A contract with Specialties, Inc., provided for the building of a two-co-ordinate electronic simulator based upòn the Columbia design. During the summer and fall of 1945 an excellent device was developed embodying general principles which should have application to many other fields. Complications arising from the termination of the war unfortunately nullified its expected utility in training Army bombardiers for the Roc tests. It was therefore delivered to Langley Field, where engineers of the National Advisory Committee for Aeronautics are using it in their research program.

Meanwhile Douglas had built their own one-co-ordinate simulator, a three-wheeled cart on which the operator rode while looking into a mock television screen. The cart was guided by mechanisms having scaled characteristics of the real bird. A target moving along a cable simulated evasive action or cross wind. The reduced scale crowded 15,000 feet of trajectory on a 150-foot floor space, but the time of fall was reproduced correctly.

With either of these devices some means was necessary to indicate to the bombardier a program for aiming the bird. Remembering the parabolic trajectory, one realizes that in a perfect uncontrolled drop the target will appear directly ahead — centered on the television screen — only at the end of the drop. Until that time it has been moving at a decreasing rate up from the bottom of the field of view. Keeping the target centered on the screen therefore warps the normal trajectory into a pursuit curve. In the case of target motion (or cross wind) this automatically insures a miss. Julie at Columbia built an electronic, and Douglas a mechanical, "regulator," to superpose on the television screen a reticle to indicate the correct position of the target. If the bomb was so aimed and controlled that the target image remained centered in the reticle, the normal parabolic trajectory would be followed and a hit would ensue. Numerous test runs on the Douglas cart proved the value of this system.

A few successful drops were made of MIMO-equipped Rocs. None of these occurred after the regulator regime described above had been developed, so results were not perfect, the smallest miss being 69 feet.

The Army was planning extensive tests at Wendover when the cessation of hostilities caused termination of the project. Thus a well-conceived and well-executed program produced an expensive bird which failed to see combat — nevertheless, the efforts expended are far from wasted if proper attention is given to the Douglas reports. In the course of this work many principles have been elucidated which are fundamental. Solutions of some of the difficulties inherent in all guided missiles have been reached, while some of the problems remaining unsolved have been more precisely defined. Roc was a unique bird. Its lessons must not lie unheeded.

CHAPTER XXVI

GUIDED-MISSILE COMPONENTS

DIVISION 5, if it learned anything, learned by hard experience that a guided missile is a complex mechanism of interrelated parts. The design of any one component affects and is affected by the design of any other. It was only those birds which were designed as units rather than assembled from separate components that ever met with any success in the field. Nevertheless, work was needed on components which showed promise and possibility of utilization in combat missiles. It was not until several parallel lines had been explored that it was possible to determine which should be continued and which abandoned. Section 5.5 was the sponsor for the subsidiary projects on components.

For no good and sufficient reason, one homing project seems never to have been blessed with a code name. It originated very early, reached a satisfactory conclusion, and was in its turn prevented by the termination of hostilities from helping to eliminate Japanese. Electro-Mechanical Research, Incorporated, of Houston, Texas, was a small organization set up as a nonprofit subsidiary of the Schlumberger [Oil] Well Surveying Corporation to build military devices. H. G. Doll, President of E.M.R., an officer in the French Army until the fall of France, had noted during his military career the need for devices he felt capable of inventing.

Thereupon Doll and Charles B. Aiken worked upon these ideas, one of which envisioned a homing bomb. In spite of hindrances, definite advances were made. The deterrents were such things as lack of priorities and consequently materials, and more effective, lack of enthusiasm on the part of Army and Navy. Some of this was perhaps natural. Security officers were especially anxious to look into the reason why aliens (Doll and one of the engineers) wished to spend their own money helping the United States Army.

The advances were: a spiral scanning system for locating and locking-on the target, aerodynamic-design and trajectory calculations showing

the feasibility of the whole scheme, an anti-roll stabilizer which was novel in those days in that it worked, and a course-predicting mechanism to permit an interception course.

By the end of 1942 about $80,000 had been spent on the various E.M.R. developments. Manifestly all the good will there was couldn't allow continued expenditure without some assurance that funds would be available from some other source or that in the event of success the project would find welcome at the hands of the Services.

At the suggestion of Assistant Secretary of War Lovett, Aiken interviewed Lieutenant Colonel A. Nyman of Wright Field and Harold Richmond and J. C. Boyce of Division 5. All three showed definite interest, and an NDRC contract ensued.

The first vehicle planned by E.M.R. was a semiglide bomb greatly resembling the Douglas X-Roc. Conferences between Aiken, Bemis, Nyman, and Boyce, among others, revealed that most of what E.M.R. was doing duplicated the efforts of one or more of the other Division 5 contractors. It was therefore decided that the best utilization of the evident talents of the E.M.R. crew would be in the building of a different type of scanner, one which might operate on a semiglide bomb or on one of the Army's large brood of glide bombs.

Most scanning systems for directing the homing of an object through space have utilized some sort of a rotating scanner. This is simple to build, and the use of simple commutators will allow identification of the quadrant in which the target is located.

A glide bomb might alternatively use a to-and-fro scanner for right-left indication and avoid some of the difficulties inherent in rotary scanning near the horizon. This was a field which seemed to promise profitable results to the explorer, so Aiken was directed to build such a scanner. His first demonstration of a preliminary model showed that this scanner was good, insofar as it gave good target indication at usable distances when aimed at ships in the harbors. As a military device, however, it had evident shortcomings. It was far too large and heavy for an airborne weapon. Aiken himself dubbed it the "Mack truck model." In addition it needed refinement.

The most successful contribution of the E.M.R. group was an extensive study of the relationships between the details of construction of receivers and their sensitivity and speed of response. This work was a

major contribution to basic theory. Field work was carried on under difficulties in a small cottage on the shore of Galveston Bay. What few neighbors there were naturally felt curious, and even suspicious of this secretive group of men who brought elaborate equipment, with great precautions, to study shipping entering or leaving Houston harbor.

The final scanner utilized the results of the research and gave very good performance. It was demonstrated at a station overlooking Boston Harbor and later at Wright Field. The outcome was an Army contract for development of a glide-bomb control. This latter work has since been completed successfully.

In retrospect this project appears to have been somewhat diffuse. Some excellent work was done by some very competent men. Since most of the work previous to the NDRC contract was done privately and independently, it duplicated similar work under the Felix, Razon, and Roc programs. This duplication was allowed to continue under Division 5 in order to provide insurance against failure of the other devices to materialize. In addition, of course, the scanner was made available to the Army for use in their glide-bomb programs.

There was one other offshoot of the glide-bomb project which received some attention by the Division but which was abandoned after some effort had been expended. The only attempt known to have been made by any nation to use expendable animals to control the bombs, on which the animals rode to the target, was by the Japanese. The Baka bomb and the kamikaze suicide plane are well-known as examples of devices operated by a human passenger. Much less well-known is the fact that lower animals were considered by NDRC and eventually rejected.

This story starts with a gentleman who approached the research director of General Mills with a visionary idea for using dogs to steer submarine torpedoes. A call to B. F. Skinner of the University of Minnesota of course quieted that idea, when Skinner pointed out that an automatic device could substitute for the dog. Thinking over the matter, however, Skinner realized that it was true that there are many targets which require a brain to recognize them — as a particular structure in the middle of a city.

Eventually, he settled upon the pigeon as the most suitable expendable

pilot. Pigeons could be trained easily and were not too much of a problem logistically. The problem was therefore suggested to Tolman in May 1941. At that time it was rejected as too vague and visionary. After some experiments had been tried, a second proposal to NDRC was also rejected in March 1942. That summer General Mills appropriated $5000 from their research funds to support the project on a small scale. Then in February 1943 movies of the experiments were shown to Division 5 and other NDRC personnel in Washington.

By this time, prospects seemed good enough to justify a trial, and Division 5 approved a proposal for $25,000 for the project. Experiments continued throughout the year.

The target image was projected on a ground-glass plate while the pigeons pecked at the desired spot. The peck-plate was mounted on gimbals, with pneumatic pick-off to register tilting. If the pecking occurred in the exact center of the plate, the target was head on, no tilting occurred, and the bomb remained on an unchanged course. If the target image moved off center, pecking tilted the plate, and the appropriate controls were brought into action to restore the bomb to its desired course. Final demonstrations were made in February and April, 1944. The psychological factors had been handled with great ingenuity, and there seemed no doubt that the pigeons could be trained to do their job in spite of distracting noises and discomfort. Due to a combination of causes, among them the logistics of pigeon supply and weakness of mechanical engineering of the device, the Division by a divided vote decided to abandon the project.

CHAPTER XXVII

RADIO-CONTROLLED BOMBS: AZON AND RAZON

IN OCTOBER 1940 Section D-3 was supervising a few contracts and industriously seeking more fields in which it could be of assistance. All the Section members had made a profitable visit to Wright Field on September 16, 1940. Colonel (then Captain) George V. Holloman had asked that they look into the matter of a target-seeking control for the Army glide bomb. Although there had been some discussion of a high-angle bomb, there was not very much interest. It was the general opinion that the conventional high-angle bomb fell so rapidly that there was little hope of improving its accuracy by any manual guiding.

Subsequently, however, Johnson, Grondahl, and J. P. Molnar, Technical Aide of D-3, changed their attitude as a result of a visit they paid to the Hazeltine Electronics Corporation Laboratories. There, W. A. McDonald showed them a television camera and receiver which Hazeltine had developed for use in falling bombs. (This had been conceived independently of the television-equipped flying torpedo which RCA was already studying for Section A-E.) At this time the television equipment had been built and was available for laboratory demonstration. It was believed, without much evidence, that if such equipment were built into a high-angle bomb it would be very easy for a bombardier to watch the television screen, to control the bomb by radio, and to score a direct hit on the target.

As a result of this demonstration, the Section presented to NDRC a proposal calling for the sum of $45,000 for development of the television and the radio control, and some aerodynamic study of the bomb. The Director of OSRD decided that the project should be pushed even though it paralleled the RCA program.

Let us stop at this point to consider the implications of this apparently innocent proposal. Theretofore all efforts toward improving the accuracy of aircraft bombing had been directed toward refinement in the

design of bombsights and associated equipment, and toward the design of bombs having stable trajectories. These efforts had been successful. The results were not as good as the public, or even many officers, expected, but they were very good. The combination of the Norden bombsight with the standard M-44 and M-65 1000-lb. bombs was to prove its worth in combat some three years later. Accuracy thus obtainable was nevertheless far removed from the legendary "bomb in a pickle barrel from 25,000 feet": in fact at times American bombing was very bad. Yet, under favorable conditions, a good percentage of the bombs dropped could be expected to hit within the area represented by a large factory. But even assuming a perfect bombsight and complete information on the aerodynamic characteristics of the bomb, atmospheric disturbances still make it impossible to predict exactly where the bomb will hit. This project of D-3 was the first work actually directed toward elimination of all dispersion by correction of the trajectory of a high-angle bomb during its flight.

A. V. Loughren and D. E. Harnett, of Hazeltine, were in charge of the development of lightweight television equipment, while Molnar made the first studies in connection with the design of suitable controls for the bomb. The general principles adopted for the guidance of the designers were: (a) there must be sufficient control by the rudders so that the bomb could be guided to a predetermined target; and (b) the bomb shape should deviate as little as possible from the standard American 1000-lb. G.P. Bomb. This latter limitation required that full use be made of the bombsight, with the rudders used only to the small extent necessary to change a miss into a hit.

In retrospect it is of interest that all concerned were convinced that roll stabilization would be unnecessary, that there would probably be no rolling at all of the bomb or at worst only a negligible slow roll. It is even possible that foreknowledge of the troubles that were to develop in eliminating roll might even have prevented D-3 from undertaking this project.

The first demonstration of the television equipment in flight was not impressive. The resolution in the picture (120 lines) seemed satisfactory to the Army representatives but was disappointing to NDRC. It was not possible, for example, to identify a small house from an altitude of 9000 feet. Two weeks later, with a larger viewing screen,

better resolution was obtained. And so it went, steady plodding, with results that were not spectacular but usually encouraging.

So far there had been only vague consideration of the exact nature of the bomb itself. For one thing, there was very little information on hand. As has been mentioned, Dryden, of the National Bureau of Standards, had published a report in 1927 on *Aerodynamics of Aircraft Bombs* which tabulated the available data. Using this, Molnar calculated that control surfaces of allowable dimensions should give sufficient maneuverability to deflect the bomb by $\pm$ 10°.

Progress in television was promising, but there was freely expressed skepticism as to the possibility of any successful control even with good television. The Section therefore decided that it was necessary to get direct evidence to settle some of these disputes. E. A. Eckhardt offered the assistance of the Gulf Research and Development Company Laboratories in fitting a standard-size bomb casing with a motion-picture camera. Pictures taken looking forward during a drop would allow determination of (a) rate and amount of yaw, (b) rate and amount of spin, (c) appearance of the target throughout the drop, (d) time available for action by the bombardier. In an ill-advised attempt to economize on cameras, it was decided to fit the bomb with a parachute which would open at about 2000 feet elevation and rescue the camera. This, of course, prevented any observation of the last, and crucial, part of the flight.

Camera bombs were built during the summer and fall of 1941. The landscape was photographed at 64 frames per second by a 16-mm. camera, having a mirror so arranged as to superpose on the field of view illuminated cross hairs and the time indication from a stop watch.

This small contract, originally calling for $5,200 for seven months, was destined to be extended and expanded until $1,140,200 was spent over a period of four and one-half years.

With a similarly deceptively mild introduction, M.I.T. undertook on May 1, 1941, to spend $3,700 making some aerodynamic studies leading to design of the air frame. Under the direction of Shatswell Ober and John Markham of the Department of Aeronautical Engineering, wind-tunnel studies were to be made on various models. The first work involved verification of the Dryden data mentioned above and calculation of various trajectories using those data.

It was immediately apparent that if the television camera were mounted rigidly along the bomb axis, the bomb could not be steered satisfactorily. Following a free-fall trajectory, a perfectly dropped bomb would gradually nose over, the target first appearing on the bottom of the screen and reaching the center in the last few seconds only. Variations in procedure which allowed more time for steering were considered, but they were shown to entail missing the target by large amounts.

Alternatively, therefore, the television eye must be made to look in the direction of travel, along the tangent to the trajectory. One way to accomplish this was by the use of vanes, or "ears," projecting into the wind stream. Using this procedure, the trajectory could theoretically result in a perfect hit.

All this work so far was based upon the standard bomb without control surfaces. Then came the crucial questions: could adequate controls be fitted into the space available, and would a controlled bomb be stable? It was some months before these questions were answered in the affirmative in the wind tunnel and three years before field tests gave complete confirmation. By that time television bombs had given way to Azon.

In the discussion of glide bombs we have seen that RCA had been working on a television-equipped controlled aerial torpedo, under the direction of Section A-E. In April 1941 an RCA television unit was demonstrated to Section D-3, in the hope that it could also be used in the high-angle dirigible bomb. It proved to be too big, but its performance was definitely superior to that of the Hazeltine unit. The latter was particularly bothered by shading and by lack of contrast. As the situation in Europe grew more serious, and the tempo of NDRC activity quickened, it was evident that time was more important than possible duplication of effort. NDRC therefore authorized a contract with RCA to develop television equipment of small size and high sensitivity and reliability.

By August the preliminary results were encouraging, so M.I.T. was authorized to build radio-controlled bombs suitable for dropping with television equipment. These were to be controlled in a cylindrical coordinate system. In this system no attempt was made to prevent roll,

merely to restrict its rate to a value of the order of one-half revolution per second. There was only one set of control surfaces for steering. Ailerons for control of roll were provided in a plane normal to that of the steering surfaces. For steering, the bomb was rolled so that a plane normal to that of the rudder contained the target, the bomb, and the aircraft. The action of the rudder then gave the bomb an angle of attack to produce lift, and consequently an acceleration in the desired direction.

If a turn to the right was desired, the bomb was rolled until the rudder was vertical, and then the bomb was yawed. For a dive, the bomb was rolled until the rudder took the position of conventional elevators and then the bomb was pitched.

The attractiveness of this system lay in the lack of requirement of absolute roll stability. A rate gyro could be installed to restrict the roll velocity to the desired value. On the other hand, control in regular Cartesian co-ordinates required absolute roll stabilization, which in turn demanded a free gyro. These were difficult to obtain. Furthermore, only a limited amount of control could be applied without tumbling the free gyro used.

It was to be experience, and experience alone, that proved that the easier solution was not the better. Control of a rolling bomb proved too puzzling even to experienced operators, and eventually all the missiles of this Division were to be roll-stabilized and controlled in Cartesian co-ordinates.

Primarily because of the interest of President J. H. Rand in the application of television to torpedoes and glide bombs, Remington-Rand had made some preliminary studies. When their Chief Electronic Engineer, J. J. Lamb, presented his proposal to Wright Field, no noticeable enthusiasm was aroused. NDRC proved more hospitable, and a contract was agreed upon. Remington-Rand was to investigate the reduction of the size and weight of the orthicon tube (as distinguished from the iconoscope favored by RCA and Hazeltine) to adapt it to the dirigible bomb.

By this time, the Gulf and M.I.T. projects had shown that the dirigible bomb could be built, that it could be controlled, and that if the television equipment worked properly a usable picture would be ob-

tained. The next step was the construction of ¼-scale, cam-operated models to check the accuracy of the computations. Later, full-scale models were built.

Others had also been conjuring up systems for the control of bombs. The work of the Fairchild Camera Corporation engineers, Henry Blackstone and Curtis Hillyer, on photoelectric homing controls for glide bombs, of Laurens Hammond on homing controls also for glide bombs, and of the M.I.T. Radiation Laboratory on radar, suggested that any or all of these methods of control might be applied to the bomb under development for Section D-3. Then one day a Russian-born French refugee named Constantin Chilowsky called to see Molnar. Chilowsky stated that just prior to the fall of France he had done some rudimentary wind-tunnel experiments on a small model of a guided aerial torpedo. The record seems to indicate that he had not considered most of the really tough problems involved in reduction of the idea to a workable mechanism. Instead he offered a general suggestion, leaving the details to be worked out by others.

Roughly, he proposed the general idea of a glide bomb or torpedo controlled either by radio or an attached thin-wire cable and guided by direct sight. This was known at the time as not being new — we have seen that broad idea in patents in the field dated back more than twenty years. It may be that this interview helped the plan for Azon and Razon to germinate in Molnar's mind. In any case, within the next few months most of those engaged in field tests of the dirigible bomb were to realize from their own experience that the easiest method of control was by direct-sighted steering. A simple flare attached to the tail would render the bomb visible until impact. Some months were to elapse before the problem of parallax was to intrude itself seriously. B. E. Warren was placed in charge of an expanded M.I.T. group, and activity increased. About this time hope was abandoned for the helical control system, and control in Cartesian co-ordinates adopted. Because of the difficulty of stabilization in roll, a combination of free and rate gyros was built which reduced overshoot by preventing the rate of restoration from becoming too great.

As the development got underway, it was clear that the aerodynamics information, while more inadequate than was realized, was ahead of the television equipment, and direct-sighted bombs would

be ready for test before television. The sponsors of the direct-sight technique appreciated by then that parallax offered little difficulty in azimuth, but was a serious problem in range control. Various proposals for minimizing this involved either slowing down the plane or so maneuvering the bomb according to a calculated program that in the latter part of the flight the plane, the bomb, and the target should be collinear. Steering would therefore merely involve eclipsing the target by the bomb and keeping it there. The difficulty with the latter was that the wind-tunnel studies gave discouraging reports, the amount of available control being less than the amount required to achieve collinearity.

All the preliminary drops showed the relative simplicity of right-left control as contrasted with up-down control, but most of those concerned still felt that the troubles could be eliminated in time. Against strong opposition, Grondahl argued that time was pressing, that azimuth control could be useful and valuable, that Azons could be brought into combat long before the two-co-ordinate bomb.

As more and more problems arose and as more and more bombs were needed to get data to solve these problems, it was realized that the M.I.T. shop could not be expected to produce them. Furthermore, during the spring of 1942 there was pressure from all sides for a rapid expansion of the entire program. The Joint Chiefs of Staff had created a Joint New Weapons Committee with guided weapons one of its major projects. The Navy had asked NDRC for assistance. Wright Field representatives, particularly Colonel Holloman and Lieutenant Colonel (then Captain) A. Nyman, were emphasizing that the Air Corps was greatly interested in the high-angle dirigible bomb, full responsibility for which lay with D-3.

Since it was agreed that Gulf offered the best facilities for operation of a central laboratory, the Gulf contract was extended, with $2,000,000 to be spent within six months. Ralph D. Wyckoff was put in charge of the program, with Molnar as his assistant.

The M.I.T. project was also continued, as were the television contracts. Gulf was given primary responsibility for the entire program, but M.I.T. continued to supply wind-tunnel data and assist in design.

Along with the widespread interest mentioned above, there came the feeling that efforts needed to be co-ordinated. On June 5, 1942,

therefore, the Joint New Weapons Committee created a temporary subcommittee consisting of Ralph D. Booth, Captain Oscar Smith, U.S.N., and Colonel John T. Murtha, Jr., USAAF, to study the guided-missile program and to recommend an over-all American program of research and development in this field.

On September 17, 1942, after extensive studies, it presented a summary report which was expanded on October 29. The conclusions were these. (1) Controlled missiles showed merit. (2) Three vital problems were still unsolved, control at velocities close to the sonic regions, stability, and adequate available control. (3) Failure to consider the cost-result ratio had led to sponsoring of some work of doubtful value. (4) A central control agency should be created to co-ordinate and accelerate the present work. (5) Tactical soundness was a factor unduly neglected by some of the workers.

Another JNW subcommittee, on Radar Research and Development, reported on November 28, 1942. In contrast to the lack of enthusiasm of the Booth Committee, they reported that "it is our judgment that the further development and test of the high-angle dirigible bomb should be vigorously prosecuted." Their feeling was that the television bomb was further advanced, but that the direct-sight weapon was still hopeful.

These two reports produced action. In October, Bush appointed H. B. Richmond Chairman of the special committee recommended to co-ordinate the NDRC guided-missile program. Before he could render a report the reorganization of NDRC had taken place, and his committee, with the addition of J. C. Boyce, became Division 5. In his report to JNW, Richmond gave the high-angle dirigible bomb very low priority. The project had been active for thirty months, and adequate control surfaces had been designed and tested, but proof that accuracy would be improved, or even that roll stabilization was possible, was lacking. More tangible results were justifiably demanded by the Division and the Services.

Placed under Section 5.2, the project was consequently given intensive study by its proponents. More aerodynamic data, both from wind-tunnel and drop tests, were needed, and better presentation to the Services.

In February, the problem of parallax seemed to the Army adequate to eliminate Razon, the two co-ordinate bomb, from consideration,

leaving television and heat-, light-, or radar-homing bombs as acceptable. Grondahl was still supporting Azon, without receiving much encouragement.

As the aerodynamic work progressed, it became evident that a conventional design, in which plane fins were disposed in a cruciform fashion, aggravated the problem of roll stability. Wyckoff, in particular, was convinced that the bomb with cruciform tail, when subjected to simultaneous yaw and pitch action, must necessarily roll if the ailerons were kept within the agreed size limits. As an alternative a cylindrical shroud at the tail was proposed, later to be revised to octagonal.

It was about this time that Gulf set about an elaborate program of complete instrumentation of field tests which was destined to pay big dividends.

In the meantime the television work was progressing. A contract had been given to Farnsworth to develop their image dissector system. Results with the Hazeltine equipment had been spotty, but two drops using the RCA television transmitter and receiver gave very good pictures. On one of these, the drop was late, and the engineer on the ground, watching the picture on the monitor scope, was horrified to see himself centered in the picture, indicating that he was the effective target. Manfully sticking to his job, he was rewarded by having the bomb hit a short distance away.

It may be pertinent at this point to mention that as designed the television bomb had virtually no room for explosive.

A number of field tests produced results which began to fall in a more satisfactory pattern. With improved gyros, it could be proved that single-co-ordinate control was possible, but existing ailerons could not prevent roll when two-co-ordinate control was tried. Gulf therefore built twelve Azons.

Of these 12 Azons, five failed to stabilize in roll, due to inadequate aileron power, six more were not successfully controlled, but one was steered within 20 feet of the road used as a target. In retrospect it can be seen that this one successful drop, at a time when Service (and much of NDRC) interest was at a very low ebb, marked the turning of the tide. Sixteen more Azons were dropped, technically in accordance with the directive of Division 5 that all such work was to obtain data in furtherance of the television or homing-bomb problem. Actually,

they also bore the hopes of those who thought that the decision to terminate the direct-sight project was premature and ill-advised.

When these Azons were tested, the average error of those controlled was 42 feet. The average error of 8 uncontrolled bombs dropped simultaneously was 1215 feet. This was definite unimpeachable evidence that Azon had tactical value. The result was that the Division restored Azons to a position of No. 1 priority within its program, followed by Razon, while television for high angles was shelved and homing was deferred.

A contract was arranged with the Union Switch & Signal Company to work out the production design for the 1000-lb. Azon. The final production design was for a tail to be mounted on a standard 1000-lb. bomb, with gyros, batteries, radio receiver, flare, and antenna, all contained within or on the tail structure. Gyro-controlled ailerons stabilized against roll, and rudders allowed steering. A simple on-off control system using a superregenerative receiver was adopted.

Eventually, Azon was included in an Army demonstration of guided missiles held before a large number of officers at Muroc Lake, California. The first Azon hit the center of the target. The second was deliberately steered 500 feet off the target and then brought back to an impact within 35 feet of the center line. The outcome was a request by the Army for 1000 Azons for combat use.

The long history of radio control of vehicles of all kinds has already been mentioned. In the light of such a wealth of existing equipment the Division felt that the bomb itself might be a problem but that the radio link was merely a procurement job. It was realized of course that sooner or later problems of security and enemy interference would intrude. John Hays Hammond, Jr., had been emphasizing the secrecy aspect of the matter from the very beginning of his work. But by and large it was believed that all that was necessary at the beginning was to select a suitable transmitter and receiver and install them. This optimism was to hinder the introduction of Razon.

During preliminary tests the RC-186 transmitter already adopted by the Army was used and proved satisfactory. The receiver was a different story. A superregenerative receiver possessed attributes which were desirable during the test program, and was so used. For combat use its instability and selectivity were major drawbacks.

Section 5.5, composed of J. C. Boyce, A. C. Bemis, E. M. Lyman, D. B. Sinclair, and J. D. Strong, with E. W. Phelan as Technical Aide, had been assigned "Mechanisms" upon the organization of the Division. Its function was to serve the other Sections by developing components. The radio link was an example, and three contracts represented different phases of the problem.

For improvement of secrecy, an earlier D-3 contract was taken over under which J. H. Hammond, Jr., built a very complicated coding and decoding system. He likened it to a combination lock, which is opened only by specific numbers used in a certain order. For his radio link, secrecy was obtained by a combination of amplitude and frequency modulation which had to be applied in the proper order. There is little doubt that had its use been necessary, secrecy would have been obtainable with this system, in that the enemy would have had little luck in determining how control was accomplished.

But secrecy was not the only thing to be considered. Even more immediate was the question of jamming. The enemy might not know the exact nature of the modulation and frequency combination used for control, and therefore might not be able to take over control. But by putting out enough energy in a broad band he could prevent the desired control signal from operating effectively. This was particularly true of the superregenerative receiver and much less so for the superheterodyne. In addition, of course, the superheterodyne by its sharp tuning permitted more control frequencies to be parked within the adopted band, hence more bombs could be controlled individually at the same time.

First under an engineering contract with M.I.T. and later with the co-operation of the Harvey Radio Laboratories, work was therefore pushed upon development of a more selective crystal-controlled superheterodyne receiver. Quite a number of the Harvey receivers were used during the test program.

In the spring of 1944 the Section attempted to avoid for Razon the troubles that had arisen through failure to prepare in time for Azon a radio receiver specifically designed for the job in hand. A contract was arranged with Philco under which they agreed to design for production a superheterodyne receiver using the principles of the Harvey instrument.

The Philco receiver gave good performance but met with Signal Corps opposition on the ground that it contained critical components in unprocurable quantities. It is fair to say that some of the grounds advanced were specious, to be forgotten later. The main effect was loss of time. Eventually, under an Army contract, the essential features of the NDRC crystal-controlled superheterodyne receivers were included in the AN/CRW-7 manufactured in quantity by Delco. In the meantime, in order to have receivers for Azon, Union Switch ordered from the General Instrument Company superregenerative receivers substantially of the Gulf design.

As could be expected, the first pre-production Azons from Union Switch & Signal had bugs which were troublesome. They rolled — and the ailerons had to be linked mechanically to stop this. The radio failed — and study of microphonics at Harvard and at Langley Field failed to find the cause, but a thorough filtering of the power supply and raising of the threshold voltage of the receiver cured this. The flares ignited erratically — and studies at Wesleyan University and Picatinny Arsenal were necessary to make them dependable. The storage battery had to be redesigned — and the Willard Battery Company produced a better battery.

Meanwhile, the Army had organized an experimental combat squadron to take the weapon to the Mediterranean Theater of Operation. On March 25, 1944, this group left for the Fifteenth Air Force. Abner Wollan, accompanying them as Technical Observer, reported that experience with the weapon was definitely successful. Missions against the locks on the Danube River at the Iron Gate, and one against the Avisio Viaduct just south of the Brenner Pass, were particularly gratifying. Reports were that on the latter mission no bombs hit the viaduct other than Azons, and that one Azon from each airplane did hit the viaduct, putting it out of commission for a considerable period of time.

The early reports on the use of Azon were so impressive that immediate action ensued in Washington. A second squadron of Azon-equipped B-24 Liberators under the command of Major R. K. Holbrook, assisted by Major J. H. Rand, had left for the China-Burma-India Theater on May 2. While they were crossing Africa, orders were issued transferring them to England, where last-minute preparations were

under way for D-Day. Azon had passed its minor-league tryout and was due to play in the big league.

Based in England, and attached to the Eighth Air Force, they were assigned bridges on the Normandy supply-routes as their targets. This of course was the famous isolation of the Normandy beachheads, using all available methods of bombing. Weather prevented much effective use of Azon, but six missions were flown successfully. One bridge at Tours was destroyed, important locks in a canal were destroyed, and direct hits were secured on seven other bridges. This was much better than could have been expected from an equal number of standard bombs. Nevertheless, enthusiasm waned.

For one thing, pilots objected to continuing the bombing run the extra thirty seconds until impact. Secondly, with existing Air Force credit based on tonnage of bombs dropped rather than on damage done, Azon was definitely at a disadvantage. If, following accepted practice, six Azons were dropped in salvo, five must necessarily miss while the sixth might hit. If on the other hand, Azon was used as recommended, one per ship on each pass, tonnage dropped would decrease. In many headquarters, tonnage was paramount, regardless of the number of targets destroyed. When air crews are rated upon number of missions or upon number of tons dropped, war is put on a mass rather than a quality basis. Both the Division and Army survey officers have emphasized that as long as commanders insist on putting their men on a piece-work basis, the unit of measurement should be the target destroyed rather than the explosive delivered. This argument however found few sympathetic ears in Air Force headquarters.

In addition, some of the officers concerned became interested in other projects to which the Azon equipment was diverted. After some 2500 Azons had been expended in the Mediterranean Theater and 500 in the European Theater, the project was forgotten.

In November a new squadron was sent out to Burma to replace the squadron that had started in that direction but had been diverted to England. There operating against Japanese supply lines, they used up the entire available stock of Azons with phenomenal effectiveness: Air Force officers in other theaters then issued frantic requests for Azons.

Consider these results — on the first 7 missions, after long, tiresome flights over the Bay of Bengal, 116 controlled Azons were dropped

through considerable antiaircraft opposition on narrow single-track railway bridges; 35 direct hits were obtained, with 15 bridges destroyed. In fact, the Azon ships on one mission found themselves in the enviable position of Lord Jeffrey Amherst. Having destroyed all their assigned targets with but half their cargo of bombs, they "looked around for more when they were through." The Taungup Pass and Okshitpin bridges had survived many attacks without receiving a single hit from standard bombs, but they were just bridges to the Azon team and down they went under several direct hits. On another gala day 27 bridges were destroyed by one mission.

Thanks largely to the Azon crews who first destroyed the bridges and then blew up the substitutes as fast as they were built, not a single load of supplies reached the Japanese in Burma after December 1944 without at least five unloadings, ferry trips, and reloadings. For all practical purposes, effective enemy transport ceased to exist.

As a natural outcome of this success, manufacture of Azons was resumed, with the Fifth, Tenth, Fourteenth, and Twentieth Air Forces all clamoring for equipment and technical assistance. Computations by the Applied Mathematics Panel based upon test and combat results with Azon have shown that the average number of hits on a representative line target, per 100 bombs, should be 25 for Azon and 1.7 for standard bombs. Furthermore, Azon-equipped ships can be used for any other type of action when weather or target conditions are not suitable for Azons. The additional risk to the bombing crew due to lengthened bombing run has been calculated to be negligible as contrasted with the additional effectiveness of the guided bomb. Hence, after a long period which definitely included three trips through a cloud of disfavor, Azon can be claimed as the outstanding success of Division 5.

In fact, it has been authoritatively stated that the destruction of the Avisio Viaduct and the bridges in Burma more than repaid the entire cost of the NDRC high-angle bomb project, which totaled $2,171,250. Let us here, therefore, give credit to L. O. Grondahl, Chief of Section 5.2, the other section members, E. A. Eckhardt and B. E. Warren, and A. J. Wollan, Technical Aide, for their work in carrying this program through from a nebulous idea to a formidable weapon.

It will be remembered that when the high-angle dirigible bomb was first proposed, it was considered axiomatic that it should be controllable

both in range and in azimuth. As has been mentioned, there were three important factors in the change of emphasis from two-co-ordinate control (Razon) to single-co-ordinate control (Azon). The first of these, and primary as far as Gulf was concerned, was the question of roll stabilization. Although this is still debated, Wyckoff was convinced that roll-stabilization by ailerons was impossible in the presence of simultaneous application of pitch and yaw control. On the other hand, roll could be prevented very satisfactorily if only pitch or only yaw control was applied.

Secondly, all observers agreed that azimuth control was all that was needed for use against a long narrow target, and obviously design problems were simplified.

Lastly, and of course inherent in the general statement that Azon was easier than Razon to steer, was the fact that the parallax problem had not been overcome. By this is meant that as the bomb falls it has a forward motion virtually the same as that of the plane. The bombardier therefore could not look along the trajectory and decide on the probable impact point. Instead, he saw the bomb moving along at an increasing distance almost directly below him. Since he could not know the distance the bomb still had to fall, at any given instant, he had no direct way to determine the probable point of impact in the range co-ordinate. On the other hand, it was a simple matter to project the path to determine the needed amount of azimuth steering. If these objections could be eliminated, of course, it was easy to recognize the advantages of Razon. It could do anything that Azon could, and in addition compensate for range errors. It will not be forgotten that there were a number of other projected applications of two-co-ordinate steering which were simultaneously under study at Gulf, along with direct-sighted radio control. One was the television bomb, others were to carry various forms of automatic target-seeking devices, including radar.

At one stage, in order to eliminate the violent roll torques which resulted from simultaneous application of pitch and yaw controls on the bomb with a cruciform tail, Gulf started building, from M.I.T. designs, a fin structure having circular symmetry. Such a structure, which was virtually free from roll torque, could be steered by an auxiliary tiltable cylinder, and was shown by test to be much more satisfactory from the aerodynamic point of view. On the other hand, it ran into the stone wall

of space limitations. It was necessarily larger, and the tiltable control cylinder proved to require intolerably large power for its control.

It was a simple matter for Gulf to modify this structure to eliminate these difficulties. Six bombs were built with a cylindrical lift shroud surrounding the bomb at its center of gravity, and an octagonal tail for stabilization and steering. Movable flaps on the trailing edge of the tail surfaces served as elevators and rudders for steering. Interestingly enough, these were the bombs, dropped with fixed-pitch control and steered in azimuth, whose drop tests so excited the Army that the production of Azon was instituted. Later six bombs of this general type demonstrated very satisfactory operation when fitted with a photoelectric homing intelligence and dropped at night to home on a bright flare.

On August 25, 1943, Azon had been designed, and pilot production was under way, but guided missiles were still none too favorably regarded by most Army officers. Then the situation changed, thanks to the initiative of the enemy. On that date, in the Bay of Biscay, the Germans proved that a glider bomb could be steered successfully, sinking Allied shipping. Later, their FX-1400, which was similar in principle to Razon, was used with most unpleasant effectiveness against our ships at Salerno.

To overcome parallax, the Germans attained collinearity by maneuvering, first sailing and then diving the bomb, while the bombing plane undertook a steep climb. This procedure allowed the operator to steer the bomb so that it eclipsed the target during the last few seconds of fall. Such a procedure must necessarily result in a hit. Gulf had tried variations of this plan, but had concluded that it was not practicable, the heavy bombers of the AAF virtually stalling in an attempt to reduce their speed by the desired amount. The Germans shortly abandoned use of these weapons when a bombing raid destroyed their entire squadron on the ground.

In the interim, however, guided missiles had been proved practical in combat. Service interest was high and Gulf was directed to study two-co-ordinate steering intensively.

Being convinced that roll control of a bomb having a cruciform tail was impossible, Gulf stuck to their shroud model with a lift cylinder and an octagonal tail. Eventually, acquiescence of the military was ob-

tained in a necessary relaxation of the dimensional restrictions. It was just impossible to fit the shroud model into standard bomb racks in the same quantities as standard bombs. By careful design, Razons were to be produced which had the central lift cylinder converted to a part of the tail (see photo), which had adequate maneuverability and stability, and which would be carried in standard bomb racks. The Army was then persuaded to accept the fact that the slightly larger dimensions of Razon forced stowage such that the bomb load of the plane was somewhat reduced.

The parallax problem, as has been indicated above, was never solved by the Germans to their own satisfaction. A totally different plan, which eliminated all but one of the difficulties encountered by FX-1400 and which introduced very little that was novel to confuse the bombardier, came to fruition as the result of co-operation between Section 5.5 and 7.2.

A note from Vannevar Bush, followed by work under George Philbrick of Section 7.2, resulted in the development of Crab, which is mentioned in the Division 7 portion of this history. When H. A. Van Dyke transferred from the Franklin Institute to the L. N. Schwien Engineering Company, Section 5.5 was able to co-operate by placing a small contract with the latter firm for continuation of his work on Crab and later on Ground-Controlled Bombing (GCB). The latter subject will be discussed shortly.

Initials of significant words in the description of OSRD devices frequently become the accepted names of these devices. Climaxing this christening parade was Jag (Just Another Gadget). The discussion of Crab reveals that the only variable unaccounted for is the variation in time of fall due to control. Studies by Gulf proved that the time of fall (and consequently the range error) varied by as much as three seconds in accordance with the amount of steering required. This could obviously introduce serious errors of which the bombardier would be unaware until he saw that actual and predicted instants of impact failed to coincide.

Jag was therefore devised as an empirically designed mechanism. It operated knobs of the Norden bombsight in such a way as to compensate for this error. It was determined by an extensive series of tests that a normal probable error of 150 feet in range could be reduced to 50 feet

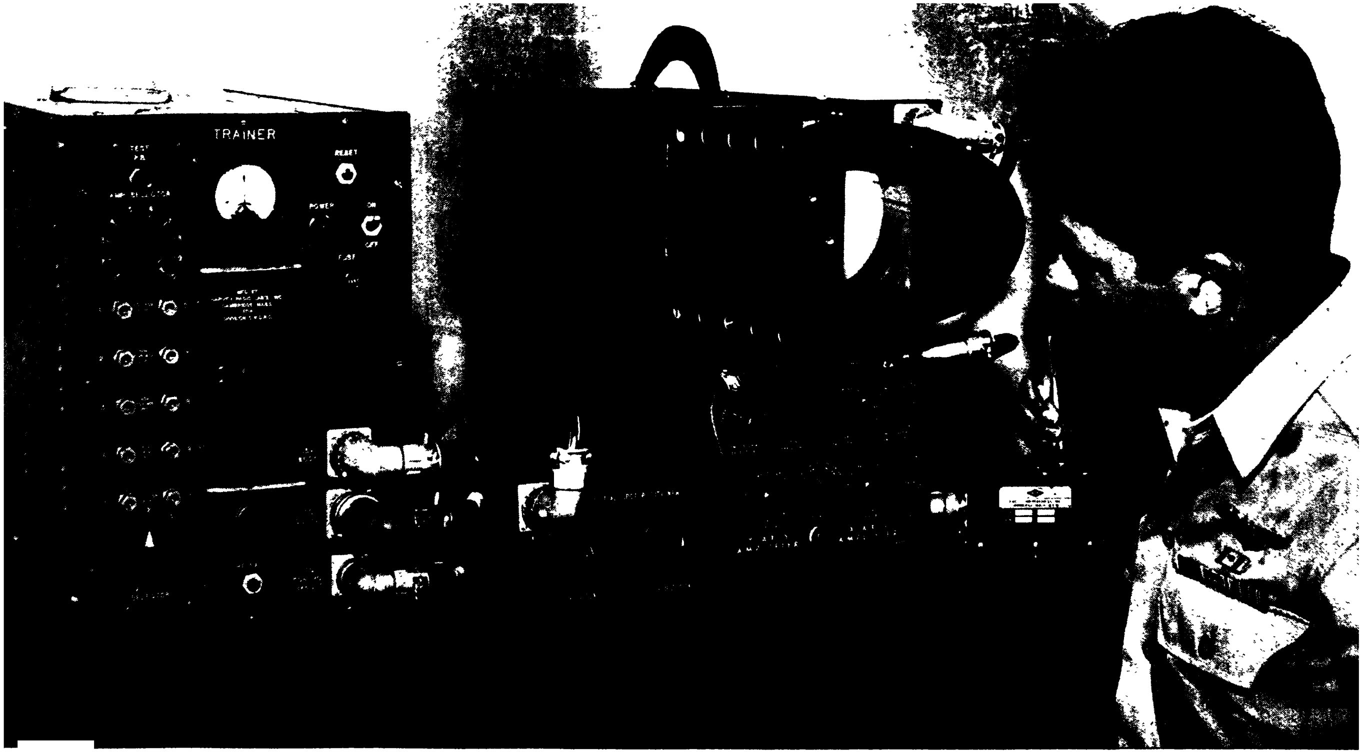
by the use of this device. Termination of hostilities at this stage of its development was to prevent combat verification of the calculated advantages.

When it was evident that Azon was to see combat, and that bombardiers would need special training in its use, plans were made for a training device. Rear Admiral (then Captain) Luis de Florez, U.S.N.R., who had planned many novel training devices, was responsible. He arranged with International Business Machines to build an Azon attachment for one of the standard A-5 bombing trainers.

This attachment turned out to be an extremely elaborate device which projected within the field of view of the bombardier a spot of light to simulate the Azon flare. The A-5 of course projects a moving photograph of the target area upon the same screen. Motion of the spot corresponded extremely well with the actual behavior as determined by field tests of Azon. This device was used at Orlando in the training of Azon bombardiers.

With Razon in prospect, and numerous A-6 trainers in production, Sections 7.2 and 5.5 again co-operated to produce an Azon-Razon trainer which could be used either by itself or as an attachment for the A-6. (This is again detailed in the Division 7 part of this volume.) The principles of electronic computation and a breadboard model of the simulator were the work of Division 7; a preliminary production model was designed and built by Harvey Radio Laboratories; and the M.I.T. Field Experiment Station group introduced engineering refinements which made the device practical. These devices have been used by the Army in training bombardiers, by the Navy in evaluation of Razon, and by both Services in public demonstrations of the principles of Azon.

The last fruit of the joint activity of Section 7.2 and 5.5 was GCB, devised in response to urgent requests from the Army that some means be developed to allow close-support bombing without danger to our own front-line troops. This obviously called for Razon if the ground forces could control the bombing. Under a direct contract with the Army and also under the Section 5.5 contract the L. N. Schwien Engineering Co. built and partly tested means for placing the control of Razon on the ground. Slight modifications to standard Battery Commander's observation telescopes allowed a pair of ground operators, one each for range and for azimuth, to preset the desired flight path of the bombs,



Harvey Radio Laboratories

Portable simulator for training operators of Azon and Razon bombs

Official Photo U. S. Army Air Forces

A bridge in Burma after a visit by an Azon squadron

then to steer them so that they followed that path. There was strong Army support for all aspects of GCB.

Just prior to the end of the war, the Union Switch & Signal Company had done an excellent job of production design and engineering of Razon. The production model had been given thorough tests at Wendover, the needed accessories were available, and all concerned were expecting to hear enthusiastic reports of combat use in the Japanese War.

For completeness, brief mention must be made of one project which started under Division 5 and is still continuing under Army auspices. Following a request initiated in February 1945, Gulf was authorized to investigate the aerodynamic aspects of the problem of control of larger bombs. This project was faced by two conflicting factors. Simple scaling up of Razon could not be satisfactory. The deflecting forces on a given bomb type increase with the square of the diameter, but the mass to be controlled increases as the cube of the diameter. Larger bombs therefore needed disproportionately larger control surfaces. This in turn increased the drag force and magnified the problem of variation in time of fall with varied amounts of control. Some preliminary models were built during the summer but necessarily failed to reach combat. Some three months after its inauguration there was also a brief flurry of activity along vaguely similar lines by another division of NDRC, but no experimental work resulted. It seems probable that future developments in this field will require considerable fundamental research.

CHAPTER XXVIII

TARGET-SEEKING BOMB: FELIX

FELIX almost reached combat. Just in time to be swamped by the news of the Japanese surrender, a cable arrived on August 9, 1945, asking for shipment of 300 Felix bombs to Guam for experimental use by the 2nd Air Force. The fact that their project therefore just missed bearing fruit was somewhat disappointing to the group collected under Alan Bemis at M.I.T., as it meant that nearly five years of work was ended without the successful combat proof that had been anticipated.

It seems to have occurred independently to many people that a bomb might be made to home on a target which an automatic device could distinguish from the background. Some of them, for example Laurens Hammond, Charles F. Kettering, Curtis Hillyer and Henry Blackstone, Franklin Offner, and F. S. Woods and Willard Buck, did some experimental work, largely on glide bombs and torpedoes. Bemis produced a device which passed its preliminary tests and was ready for combat.

Shortly after the formation of NDRC, K. T. Compton asked Alan C. Bemis of M.I.T. to become Chairman of Section D-4. Inventors were clogging the mails with ideas for military devices, and D-4 was put to work sifting out the very few useful suggestions of one class and making them operable.

At the very first meeting of D-4, in December 1940, some consideration was given to the possibility of putting a homing control on the D-3 high-angle bomb. The first thing needed, however, was to investigate the target-detector itself. Laurens Hammond was encouraged to improve his device not only as a bomb control but also as a ship detector. In this latter instrument the fact was recognized that a steamer at sea is easily distinguished by any of several detectors.

The Battle of Britain was over but the night bomber was extremely dangerous, so Bemis himself was first set to work on the problem of night detection of airplanes. As progress was made on airplane detectors, the twin facts that they were not very good and that they would have no operational use became apparent simultaneously.

Planes could be detected against a clear sky at distances of three miles, but the receiver was inoperable in the presence of vibration, as from a motor, and clouds frequently gave larger signals than did planes. Attention was therefore diverted to ship detection, which was more promising.

By this time it was quite evident that the problem had a number of ramifications all requiring extensive study. John Strong began a study of target discrimination, first at the California Institute of Technology and later at Harvard. At Strong's suggestion the Bemis group at M.I.T. turned their attention to better detecting devices. The features which particularly needed improvement were sensitivity and speed of response.

The end of 1942, when Division 5 was organized, found all of the principles which were to enter into Felix recognized, and definite progress being made toward solution of the details. Section D-4, responsible for the detector, had been collaborating closely throughout the year with D-3, working on the high-angle bomb. Shortly after the reorganization of NDRC the Felix contract was assigned to Section 5.5 while the Gulf work was supervised by Section 5.2.

After Gulf introduced their octagonal-tail bomb and demonstrated that it had adequate stability and maneuverability to carry a homing device, it was decided to build six complete Felix units for drop tests. The homing head was built and given extensive flight tests over Boston harbor and vicinity. The equipping of the Gulf bombs with automatic controls and servo systems to utilize the orders from the head proceeded on schedule.

Of the six units shipped to Eglin Field, Florida, for drop tests, one homed well, three failed to accept the intended target for one reason or another and homed on the best targets they could see, and two spun badly. At the time this seemed a discouraging record — all concerned had worked madly and the program had been run off at the time scheduled some months previously (in itself almost a unique event in this history) but only one bomb had landed where expected, while another had barely missed Colonel Nyman.

Later analysis of the camera records showed that the results were much better than they seemed. It has been mentioned that three homed on targets other than the intended one. Observation was to prove that a

very careful survey was necessary to insure that the unreasoning eye was launched in such a manner that it would see no unintended target.

Explanation of the two failures was simple but uncomfortable. The stabilizing gyros were air-driven, connected until the instant of drop to the plane's vacuum system by a rubber tube. During the excitement of the bombing run, in the limited space available in the B-23 plane, one of the crew unwittingly held his foot on the rubber tube — hence the gyros failed to spin. On such things is patience nurtured.

When these results were reported to Washington, there was justly a great flurry of enthusiasm. Brigadier General H. M. McClelland outlined a high-priority target which seemed made to order for Felix. This target turned out to be the V-1 launching sites, but that revelation came later. In the hope of attacking these sites by May, just four months off, a crash program was instituted for procurement of Felix.

Contracts were executed with Fairchild Camera & Instrument Company, Norton Company and Remington-Rand, Inc., and General Instrument Corporation, for the manufacture of the heads, tails, and electrical components, respectively.

Things had been happening so rapidly that the production job had to be started before Gulf had reluctantly abandoned hope of making the cruciform tail work on the Razon-type bomb. Part of the evaluation of the relative merits of the cruciform and octagonal empennages was accomplished with Felix bombs in tests at Tonopah, Nevada. Gulf built the first 40 tails, to be used in tests, while Norton were to build 100 cruciform tails and Remington-Rand 100 octagonal tails for combat use. As it turned out, the cruciform models spun badly in tests and were abandoned.

Gulf also merits considerable credit for their rapid aerodynamic work which determined the external features of Felix. Essentially the production model was like the Eglin Field model, but the size had to be increased to allow room for the standard 1000-lb. G.P. bomb between the head and the tail.

All seemed provided, therefore, except the target discriminator itself. Dane had been making them individually at M.I.T., but it was obvious that standardization was necessary. The outcome of a conference sponsored by the Vacuum Tube Development Committee to discuss problems of manufacture was an offer by Saul Dushman to make

available facilities of the General Electric Company laboratories for a study of the problem. With the assistance of A. J. Kling, a redesign of the necessary distinctive vacuum tube was accomplished in short order, resulting in a product which was reliable and amenable to mass production.

Results at Tonopah, unfortunately, were none too satisfactory for some time. Part of the troubles were due to the usual manufacturing difficulties—gyros connected backwards, unsoldered connections, inadequate solenoid power, etc. These were the things that caused much hard work before they were eliminated, but they were the price to be paid for the speed demanded, and they were to be expected. But there were also other more serious troubles. It turned out that the success at Eglin Field had been more accidental than real.

Fred Jennings had been engaged in a comprehensive analysis of all the details of the records of the drop tests and survey flights. Gradually he began to realize that much of the effort which had been attempting to increase the speed of response of the controls to a target signal had been misdirected. Instead, in order to eliminate large-amplitude bomb oscillations it was necessary to introduce some time lag.

A large number of factors had to be studied to determine the optimum adjustments for many interrelated factors, and it was perfectly evident that performing such tests in the field would require hundreds of bombs with no guarantee that satisfactory answers could be obtained.

Just as Gulf had found it necessary to do in adjusting their photoelectric homing bird, and as A. C. Hall had done in connection with Pelican and Bat, the Felix group set up a test table on which all the elements of control could be simulated without the destruction which accompanies a bomb drop. An elaborate assembly of gears, pulleys, wires, etc., operated a series of pens which drew upon charts an accurate record of operation of relays, elevators, rudders, bomb body, velocity vector, and eye, as the target was moved.

Systematic study of all the variables, and incorporation into the production model of the changes which these studies showed to be necessary, paid good dividends. It became apparent about the middle of the year that the crash program had been premature, and the project was restored to a laboratory status. This was no discouragement but rather

an opportunity to perfect a device whose principles were sound but whose details needed elaboration.

By fall things were clicking smoothly. At Tonopah, 12 out of 16 landed within 200 feet of the target. Production of 1000 complete units was under way at Remington-Rand. Extensive target surveys were being made. Under Commander S. S. Ballard, of the Bureau of Ordnance, an elaborate Navy program had been under way for some time to measure the signals obtained by various devices from both land and ship targets. Some of the M.I.T. personnel started an auxiliary program with the generous assistance of Wright Field to determine what would and would not serve as good targets for Felix. The time was ripe for the Army to undertake tactical tests.

A series of tests, which were destined to last for some months, were started at Orlando, Florida, first for demonstration to the Air Forces Board and later completely under their control.

At first things seemed out of hand. The change from the desert at Tonopah to the semitropical Orlando introduced problems of dampness, and it took much feverish work to eliminate these. Then there arose target trouble. As the character of the vegetation changed, different areas in the vicinity of the intended target became too attractive to the unreasoning homing eye.

It was necessary to find a unique target before the ability of Felix to head unerringly for it could be proved. After much effort an ideal location was found, an isolated small island, one of the Florida Keys. It provided a good contrast to the surrounding water and therefore served as a unique target.

The Felix test program began to meet with consistent success as production-model units were dropped on Channel Key. Ninety bombs were dropped on the island from altitudes varying from 8000 to 28,000 feet. Corrections by the homing mechanisms produced hits on the island by bombs whose free fall points were as far as 2000 feet from the center of the key.

The Bureau of Ordnance of the Navy had also been interested in Felix. A few tests were conducted by them against a ship anchored off Cape Cod, with moderate success. For better tactical evaluation, with larger numbers, tests were also scheduled against a small island near Traverse City, Michigan. A variety of complications arising out of the

termination of the war and preoccupation of the Navy with the atom-bomb tests at Bikini has so far prevented completion of this project.

Felix was the first homing bomb of its type to be tested. It was the only one to reach the status of a theater request. An elaborate target survey had shown that the Japanese islands held many spots well-adapted to attack with Felix. Adequate testing had eliminated troubles until Felix was declared reliable and adequate. The war's end came just too soon for combat verification of the predictions of its sponsors.

Division 5 sponsored two weapons which reached combat, together with two others which were almost ready. Under the leadership of H. B. Richmond and later H. H. Spencer, definite attempts were made to comply with the desires of the Service personnel associated with the various projects. In some cases this can be seen in retrospect to have limited progress. Changes in assignments put new liaison officers at times in the uncomfortable position of having to make decisions before they became thoroughly familiar with their duties. In the end, however, a smoothly operating team of representatives of NDRC, Army, and Navy co-operated in advancing the fields of guided missiles. This was their responsibility and they fulfilled it. True it is that during the early stages of the war, when we were fighting a retrogressive, defensive war, there was need for stout-hearted support by the liaison officers. It took fortitude to provide field-test facilities when those facilities had to be withheld from combat officers clamoring for every available plane and also had to be shared with the Services' own projects. Let us here record thanks that this was done and that time proved its wisdom. Certainly Azon alone repaid the Army and OSRD for all the efforts expended upon the guided-missile program.

CHAPTER XXIX

EDITOR'S POSTSCRIPT

If ANY common factor of success can be found in the various technical developments described in this volume, it is the factor of quantitative tests. This is particularly true in the complicated dynamical situation found in fire-control systems and in some forms of guided missiles. Improvements in proximity fuzes also would have been much more difficult to obtain without the quantitative data obtained in vertical-firing tests and by other means. OSRD projects were by no means above criticism on this score, nor did they originate the idea of quantitative tests or all of the methods used in obtaining data. But OSRD groups and contractors may claim credit for emphasizing in the development of new weapons this fundamental principle of engineering and of science — "Measurement is the basis of knowledge."

Elaborate instrumentation is often required to collect and record the quantitative data. Without it a test can show whether or not the particular sample of a new weapon performs according to expectation, but no indication is obtained as to the detailed causes of failures or the direction in which improvement is to be sought. Tests conducted without adequate instrumentation often lead only to prolonged and needless debate. In such arguments, the opinion of ranking members of the test group may unduly influence the decision, whether they be civilian or military.

Closely connected with the value of quantitative measurement is the use of simulative devices. These are particularly important where an ordinary test is destructive, or involves the co-operation of large groups of personnel, or includes variables not all of which are under control. The reader will recall the use of the dynamic testers in the development of fire-control directors, the Texas tester for airborne-gunnery devices, the test devices used in guided-missile developments, and corresponding techniques in the field of proximity fuzes. Simulative devices have several functions. They are an aid to the development engineer in studying the expected behavior of a new device, either by itself or in

combination with various operators. With them, many preliminary problems can be solved before actual tests begin. The engineers who will conduct the tests gain valuable preliminary practice before starting large-scale or destructive tests. This may make it possible to get useful results from the first actual tests, since the operator is then less likely to make that kind of error of judgment which destroys the effectiveness of the test. Beyond its aid in development, simulative devices play an important part in demonstrating new weapons to military commanders and finally are very useful as training devices for the personnel who will take the new weapons into combat.

What may be called "systems engineering" is often overlooked in the development of military devices. To take an illustration from the field of the Editor's personal experience: all too often the development of the various components of a guided missile was given to independent groups in the vain hope that the components so developed would function properly together. Unfortunately this tendency still persists in some quarters. Security is usually quoted as the justification for this procedure. Experience of this war has shown considerable parallelism in the independent development of new weapons in various countries. This is to be expected since the fundamental scientific and engineering principles are available to all nations. Security is wasted if a new development comes too late. Fortunately for us, the Germans and the Japanese made this sort of mistake more frequently than it was made in this country. But enough instances occurred here to waste valuable months. This point is emphasized because it is such an easy mistake to make and because the Editor has good reason to believe that some Service development groups have not yet learned this lesson.

Not all of the problems connected with new weapons are problems to be solved in the laboratory or on the production line. Very early must come a study of the over-all military worth of the weapon and of its optimum use. The development of an ingenious gadget may present an interesting challenge to the scientist. Its military value is zero unless it is likely to be used in combat, or negative if its development deflects personnel from more important projects. Here neither a civilian group nor a military group can function alone. A good beginning has been made through the various Operational Research or Operational Analysis Sections of the Services.

In assessing the military worth of a weapon or of a tactical method, quantitative data are again of prime importance. This is particularly true in air warfare. Under the stress of combat, not all of the significant data may have been noted quantitatively. The opinion of those who did not survive the operation, could it be obtained, might be very significant. When a new weapon goes into combat use, a representative sample of it should, where possible, be equipped with an appropriate type of automatic data-recording camera.

These then are some of the major technical problems connected with the development of new weapons. Other problems which may be described as administrative are often at least as difficult. Cordial and effective relations must be maintained between the laboratory, the factory, and the various military groups concerned. When all are carrying heavy loads, it is easy for misunderstandings to arise which can seriously delay the completion of a program. Differences of opinion as to the relative priority of various projects can be particularly trying. If any conclusion can be drawn from these experiences, it is to emphasize the importance of human relations in engineering.

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